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THE

# EDINBURGH NEW
# PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

## PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

## SCIENCES AND THE ARTS.

---

CONDUCTED BY

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

## *On the most recent Disturbance of the Crust of the Earth, in respect to its suggesting an Hypothesis to account for the Origin of Glaciers.* By Sir G. S. Mackenzie, Bart., F.R.S.L. & Ed., &c. Read before the Royal Society of Edinburgh on the 7th March 1842. Communicated by the Author.

The attention of geologists has for some time been directed to the present condition of the surface of the earth, in a manner calculated to bring many new facts to light, by means of a more careful examination of those already observed, and which have given rise to various speculations respecting their origin. There may be perhaps too great a tendency towards attributing to a single cause the whole phenomena of a class. While the debacle theory appears adequate to explain a great deal in reference to transported materials, there exist obstacles to its general application not yet cleared away; but, in regard to various local appearances, it is likely, when modified, to stand its ground in special cases. The same remark may be applied to the glacier theory. Professor Agassiz gave up his idea of a universal glacier having existed, and having been elevated at the same time with the Alps, probably because he could find no origin from whence such a coating of ice could have been derived. Some have appealed to astronomy for proof of the possibility of the surface of the earth having been at one period frozen; supposing that certain slow but enduring forces may have produced such changes in the relations of the earth to the sun, as would have caused great cold on the surface. But such appeals to account for ice—or, to account for a debacle, such an idea as that of a comet coming into collision with the earth, suddenly changing its axis, and giving it a new equator, are scarcely to be admitted, while there remains a chance of finding geological causes sufficient to solve our difficulties. Astronomical changes are slow, and I apprehend we need to find means for a sudden production of cold and ice; for, when we contemplate the presence, in regions beyond the tropics, of the remains of animals which required a temperature approaching to that within them, we see no indication of any but a sudden change. Hence came the notion of a catastrophe caused by a comet.

The cause which might have effected a reduction of temperature in the crust of the earth, to a degree that would account for the surface having been formerly covered with ice, or at least for the glaciers of Switzerland having, at a former period, stood much higher and extended much farther than at present, is yet in obscurity. It is with some diffidence that I venture to submit to the Society what has occurred to me on this interesting subject; but being persuaded of the importance of exciting the minds of others into a train of thinking that may perhaps lead to the development of ideas more plausible than my own, I do venture, in the hope that my conjectures may elicit from other minds something more satisfactory. I have been anxious to discover whether any such effect as a considerable refrigeration of the surface, and the production of ice, might not have followed some of those disturbances of the rocky crust, which are so plainly indicated by the present relative position of the broken masses.

The evidence that the glaciers of Switzerland have stood at a higher elevation, and that they had extended much farther than they do at present, seems to be complete. That their reduction in elevation and extent had been caused by an increasing temperature of the earth and the influence of the sun combined, can scarcely be doubted. I had therefore to look first for some indications rendering it probable that the earth's crust, with reference to temperature, had been, previous to the formation of glaciers, in a condition different from its present one, and such as would have admitted of some geological change adequate to produce such an alteration of that condition as might have originated glaciers; and, second, for means of a gradual restoration to the first condition of temperature, such as might bring about the changes that have since effected a diminution of the elevation and extent of the glaciers. I will consider, therefore,—

I. What the condition of the crust of the earth probably was before the most recent disturbance occurred.

II. The probable cause which brought about that disturbance.

III. The effects which were likely to follow.

1. It has been very generally supposed that the elevation of the primitive rocks into their present position has been caused by the invasion of granite and other matter which we find filling the veins which traverse them. This appears to have been a mistaken notion. We see lofty precipices, the faces of which exhibit veins of granite traversing gneiss, and the granite flush with the rock. Now, had the granite been, as is supposed, in a fluid state at the moment when the strata were broken and pushed upwards, so as to form precipices clear of the rest of the rock, the fluid matter must have run out of the spaces occupied by the veins. It is, then, an unavoidable inference that the granite was solid at the period of elevation; and that it is a mistake to affirm the force which drove the granite into the fissures of the rock to have been the same, in point of time, that gave to the masses their present disrupted appearance. The same observations apply to trap. Before precipices displaying trap-veins were elevated, the trap had invaded the rock and become solid. As we find trap cutting granite, we ascribe this to a period more recent than that when the granite was produced. We also find trap-veins cutting older trap, and these also must have existed before the rocks were brought into their present position, so as to give to the earth its uneven and broken aspect. As we do not find granite traversing trap, but observe numerous instances of trap cutting granite, it is obvious that the invasion

of trap has been the most recent instance of matter having been protruded from below into fissures above. Many invasions are indicated by the display of numerous varieties of granite, and of one granite cutting another; and the same is the case with trap and other invading matter. The most recent of all these appears to have been that which filled the rents of the secondary strata and beds of trap with trap; and this must have happened before the crust of the earth was so displaced as to bring its broken parts into their existing state. All the matter in veins must have been solid before being exposed to view. It is not improbable that the invasion of protruded matter to a certain extent disturbed the rocks into which it passed; but it does not appear that this had elevated the land to any considerable degree. It would rather appear that, before the rupture was effected which gave to the surface its present shape, it was very little elevated. Hence the general warmth of the climate in the temperate zone, in so far as influenced by the sun, must have been greater than it is now; and there seems to be nothing in the way of the supposition that the temperature of the crust of the earth was also greater from the influence of internal heat. These causes combined may have rendered the temperate zone warmer by many degrees than it is now; and it may have been habitable by those animals and plants, the remains of which, in certain localities, have caused much difficulty in geological speculation.

With respect to the heat from the internal source, if we reflect a moment, it will appear probable that it was formerly greater in amount, from a consideration of the phenomena which the most recent cataclysm has brought into view. The rents which we now see filled with protruded matter could not have been made by a force sufficient to elevate the rocks in which they were made very much out of their original position, because in that case the rents would not probably have been filled. It was necessary that the strata should still rest on the melted matter when they were broken, in order that the pressure (operating, perhaps, in some other locality) *might force it into* the openings, and continue until it became *solid, preventing* a collapse of the sides of the fissures. This accession of greatly heated matter would probably, if it did not increase the heat of the crust, contribute to keep it up. And as it appears that hot matter had invaded the crust at many different epochs, before it was elevated into its present shape, it seems reasonable to suppose that the temperature of climate, in so far as the influence of that of the earth extended, was considerable.*

It may be said that, according to these views, the crust of the earth should have been acquiring heat from the interior, since the period of the last cataclysm; but that we have no proof of this in the improvement of climate, but rather of the contrary. The only proof we can have, is the result of a long continued series of observations recently begun, at various depths, in mines. For it is obvious there are causes independent of the actual heat of the crust, which operate in the regulation of climate. The causes of change in atmospheric temperature are as yet obscure; and although we see certain effects following certain apparent causes, such as boisterous and wet weather coming after the appearance of the aurora borealis, we are yet ignorant of the mode of connection between the phenomenon of the aurora and terrestrial influences. It appears, however, probable, that climate depends more on operations going on in the atmosphere than on the actual heat of the crust; and that if these were absent, and the heat of the crust and the sun's rays were left to exercise their influence alone, our climate would be much warmer than it is. Hence I should not think it fair to infer, that because climate is deteriorated, the crust of the earth is not returning to its normal state, and becoming warmer. That change does take place in atmospheric conditions, appears to be unquestioned; but, as yet, no observations have been made, such as to ascertain the causes of such changes. Electricity has much to do, no doubt; but still, however numerous and remarkable the discoveries of its modes of operating have been, we are yet ignorant, and probably will long continue ignorant of those terrestrial conditions which appear to influence electrical effects, in alternately warming and refrigerating the atmosphere. I make these remarks for the purpose of shewing it to be possible, that the temperate zone may have been warmer before the most recent cataclysm, which may have effected so great a change in the electro-magnetic condition of the earth as to create alterations in atmospheric conditions sufficient to counteract, to a considerable extent, the influence of internal heat and of the sun.

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* When we consider the phenomenon of earthquakes, it seems likely that some process is going on, such as a partial tearing of the strata, and the filling up of rents; and should another cataclysm take place, such as again to break up the crust, it is probable that a fresh system of veins and dykes *will be exposed to view.*

2. It is evident that, before the disruption of the crust took place, there had been a vast exertion of expansive force. This increase of force had probably been caused by additions to the amount of vaporable matter collected and confined betwixt the crust and the greatly heated interior. When we regard the enormous masses of matter that have been thrown up from under the crust of the earth, as indicated by the vast amount of granite and other protruded matter which we find among the strata; and when we contemplate, not only the amount of trap in veins, but the very great extent of trap-beds heaped on one another, and forming lofty mountains, as seen in almost every quarter of the globe, and which are now generally admitted to have been sub-marine lavas, we cannot refuse assent to the proposition, that when all these masses were thrown up, immense cavities were left between the solid crust and the igneous matter below. Such cavities would not be vacuous, but filled with vaporable matter, probably red hot water, and exerting an enormous expansive force. That this was the condition of things appears the more probable, as nothing has been noticed, at least so far as I know, shewing that when the crust of the earth was broken up any stony matter had been erupted among and over the strata. Had there been no elastic matter interposed between the crust and the liquid matter below, we might have been at a loss for expansive force. Had there been any other power pressing the liquid matter upwards against the crust, that matter would have been forced up like lava, and have filled hollows, where we should have found it. I therefore assume, as the groundwork of my hypothesis, that there existed, between the crust

and the hot fluid below, a vast amount of elastic matter greatly compressed, and in a highly heated state.

3. Now, let us suppose the expansive force to have at length overcome the resistance of the solid crust, to have broken it up, and to have elevated the Alps, as we see them, to heights beyond the line of perpetual congelation. A vast amount of compressed elastic matter, escaping by sudden expansion at the moment of the rupture, especially if this took place in the winter season, would be followed by two special effects. The first, from the immense abstraction of heat by the escape of the vaporable matter would be a great refrigeration of the broken masses. Their exposure to atmospheric influence would also operate in reducing the temperature. The second effect would be, that the expansive force would greatly accelerate the progress of the vapour (which we may safely assume to have been almost exclusively that of water) into the higher regions of the atmosphere, where it would be frozen, and in this state fall back upon the surface and contribute still more to its refrigeration. That which fell above the line of perpetual snow would continue long unaffected by subsequent increase of temperature in the crust. That which fell below it would soon become a mass of considerable thickness and solidity, but which, in process of time, as the crust of the earth regained its heat from below, would gradually melt and disappear, leaving behind it those phenomena which have given rise to so much discussion in our time. Besides the large supply of vapour from the cavities in which it had been pent up, it does not require any stretch of imagination to suppose, that disruption of the crust would admit much water into contact with the hot matter below it, and that thus a very large additional quantity of vapour would be formed, and ascend to the upper regions, operating also as a refrigerator.*

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* At the time when this paper was read, I was not aware that Charpentier, in his "Essai sur les Glaciers," &c. printed at Lausanne in 1841, had proposed this access of water to the hot portion of the crust, at the moment of its rupture, to account for the origin of glaciers. I have used it only as a subsidiary means.

If we suppose the cataclysm to have taken place in the winter season, we can imagine the whole of the vapour falling back on the surface in a frozen state. If, in the summer season, that portion which might fall below the line of perpetual snow would be melted into rain, and then produce a formidable debacle. Indeed we can scarcely doubt that floods were produced to a great extent; but whether existing phenomena indicate floods from this particular cause remains to be ascertained. Without farther noticing this matter, we can imagine that, whatever took place beneath, there would be very large accumulations of snow above the line of congelation, such as might have given the now peaked Alps the appearance of a continuous ridge. So that, supposing the cataclysm to have taken place at the most unfavourable season, and that rain and floods were part of the results of the disruption of the crust, still it would appear probable that such an accumulation would take place in the upper and frozen regions as would be sufficient for its subsequent descent, by its own weight and other causes, to a lower level, and for its extending so as entirely to fill up the valleys in the vicinity. The refrigeration of the crust would also render the subsequent winter snows more permanent.* We are familiar with the great amount of snow which sometimes falls in ordinary seasons, and frequently enveloping living animals; and we know the solidity which such masses acquire. We may, therefore, without difficulty, conceive the effects of an extraordinary supply to have been such accumulations as may have given origin, not only to glaciers on high mountains, but to a covering of ice to the whole surface over which the cataclysm extended. The broken crust, as soon as the cataclysm and its immediate effects were over, would begin to recover its temperature by slow degrees. The records made at the present day of the indications of the thermometer at various depths in different localities, if continued, will enable geologists to ascertain hereafter whether there be any increase now going on in the temperature of the crust. Its elevation into its present shape will, however, render it impossible that the climate of the temperate zone should become what it was formerly. At any rate, the exposure of so greatly extended a surface to the agency of the atmosphere will very much retard the restoration, and radiation must also have increased with the extent of surface. Hence it is probable that we cannot look for any great improvement of climate, unless atmospheric influence shall, by some great change, be reduced.

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* *This has also* been suggested by Charpentier, as well as the probability *of a succession of cold* seasons contributing to the amassment of ice.

If I have not erred in the assumptions I have made, we have thus a catastrophe sufficiently sudden to have enveloped living creatures in a material which would preserve their bodies for ages if floated to the Arctic Regions ; or leave their remains in a region colder now than it was when they lived. Thus may the valley of the Rhone have been filled with ice, and boulders transported from the Alps to the Jura, and centres of dispersion have been formed. We may find also the origin of the older diluvium, while preparation was made for subsequent floods that have left various and, in some instances, apparently anomalous traces behind them. Agassiz may also be furnished with means to support his original inference, with this only variation, that he supposed the ice to have been formed before the mountains were raised, while I have supposed them first raised and then covered with ice.

The Society will now have perceived that I have modified my opinions respecting the glacier theory considerably. But if I had not found what I regard at present as a somewhat plausible hypothesis respecting the means by which an icy covering would be produced geologically, I should still have been in greater doubt than I am in now. While, however, I admit more fully the operations of ice, I do not give up the notion that the phenomena of the surface require for their explanation the joint action of fluid and of solid water.

## *On the Cause of the Diminution of the fall of Rain as the height above the ground increases.* By JAMES DALMAHOY, Esq. Communicated by the Author.

THE fact that a rain-gauge, placed in an elevated and detached situation, receives less rain than an exactly similar gauge near the ground, seems to have been first observed about seventy-five years ago by Dr Heberden; and has since been abundantly confirmed, especially by the observations of Dr Dalton, by a long continued series at the Paris Observatory, and by those of Professor Phillips at York.

The most generally received explanation of this curious fact is that which was originally proposed by Dr Franklin, and subsequently, but independently, by Professor Phillips.

This hypothesis is attended with several difficulties, one of which was suggested by Dr Franklin himself.* But perhaps the greatest objection to it is founded upon a fact well established by the observations at York, and exhibited in the following small table :—†

| | Year. | Depth of rain in inches. |
|---|---|---|
| Increment which the rain received while falling between the upper and middle rain-gauge, through a height of 169 feet, | 1832-33 | 4.467 |
| | 1833-34 | 3.841 |
| | 1834-35 | 4.889 |
| | Total, | 13.197 |
| Increment which the rain received while falling between the middle rain-gauge, and the gauge on the ground, through a height of 44 feet. | 1832-33 | 3.603 |
| | 1833-34 | 3.804 |
| | 1834-35 | 5.854 |
| | Total, | 13.261 |

It appears from this table that the increase which the rain received during the last 44 feet of its descent, was almost exactly equal to that which it received during the preceding

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* Manchester Memoirs (old series) vol. ii., Letter from Dr Franklin to Dr Percival.

† Reports of the British Association for 1833, 1834, 1835.

169 feet, a result with which it seems impossible to reconcile the condensation hypothesis, except by supposing the hygrometric condition of the atmosphere to vary with the height above the ground, in some abrupt and unaccountable manner not hitherto observed.

But though these difficulties attend the hypothesis of Dr Franklin and Professor Phillips, it is to be remarked that they have no reference to the *nature* of the cause which it assigns, but simply to the *degree* of its efficacy. For, while it scarcely admits of a doubt that there is a condensation of vapour on the cold surface of each rain drop, and that thereby the rain is augmented in its progress downwards; on the other hand, the observed *rate* of augmentation is such, that it cannot be accounted for, without supposing the co-operation of some other cause.

The object of the few following remarks is to suggest, as a supplement to the foregoing hypothesis, an additional cause of the increase of the rain as it approaches the ground. This supplementary hypothesis is as follows.

From the inequality of the marks which the first drops of a shower of rain make upon any smooth surface, it is inferred that the drops themselves are of unequal size. But independently of direct proof, it seems very unlikely that, in the original formation of rain, each drop should be exactly of the same size; and, allowing the possibility of such an equality, it is still more unlikely that it should continue for an instant, since the slightest disturbance of their descending motion would cause many of the drops to coalesce.

It is assumed, therefore, that even at the elevation where the rain is formed, the drops in the same horizontal stratum are not of the same size. If there were no air, each drop in such a stratum would reach the ground at the same moment; but since in reality the spherules of rain are resisted by the air, it follows, from well-known mechanical principles, that the larger drops will outstrip, in their descent, the smaller ones in the same stratum, and will overtake those in the next lower stratum. But in penetrating this stratum, they will necessarily carry before them and coalesce with the drops which opposed their passage, and so will continue to pierce the successive strata, and to augment in size as they approach the ground.

It evidently follows from these considerations, that the lower any stratum is, the larger will be the average size of the drops composing it, and the greater will be the augmentation of the drops by which it is penetrated.

This hypothesis seems to explain not only why the whole fall of rain, but also why the increment received in falling through a given height, is greater the nearer the place of observation is to the ground. It also explains why the difference between the indications of the upper and lower rain-gauges is greater in winter than in summer. For as the total increment at any elevation will evidently be as some function of the space through which the rain has fallen, that is, of the height of the cloud, where it was formed, above the gauge; and as the ratio of the spaces through which the rain descends to two gauges having a constant difference of elevation will necessarily be more unequal when the clouds are low than when they are high, it follows that the difference between the quantities of rain received by these gauges will be greater in the former than in the latter case; but Dr Dalton's table* of Mr Crosthwaite's observations, shews that the clouds are lower in winter than in summer; hence the hypothesis is in accordance with experience in this respect also.

EDINBURGH, 10*th April* 1842.

## *Observations on the Defects of Rain-Gauges, with description of one of an improved form.* By THOMAS STEVENSON, Civil Engineer. Communicated by the Royal Scottish Society of Arts.†

ALTHOUGH the subject of the fall of rain in any district of country is no doubt chiefly important with reference to the interesting but uncertain science of meteorology, yet it often forms an indispensable element in practical questions of frequent occurrence. Thus it is interesting and important in relation to agriculture; while its intimate connection with some questions of drainage and waterworks is well known to every one who is conversant with these departments of civil engineering.

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* Dalton's Meteorological Essays, p. 41.

† Read before the Royal Scottish Society of Arts on 14th March 1842.

Having been incidentally led some time ago to inquire into the inconsistent results of rain-gauge experiments, I came to the conclusion, that the great sources of error were the smallness of the recipient surface, the rim, and the position of the funnel, which offering resistance to the wind, produce disturbing eddies, and cause also resilience and dispersion of the rain-drops.

In order the better to contrast rain-gauges of the size now generally used with instruments presenting greater surface, I shall enumerate the different sources of error, pointing out as I go along the diminution of error which would in some cases result from an enlargement of the area, and adding such general remarks as may occur. I shall thereafter describe a form of instrument which seems to me to possess several advantages over those now in use.

It may in the first place, however, be proper to observe, that although there is some diversity of opinion among the best authorities regarding the proper size for gauges, yet these instruments are almost invariably made of from 6 to 12 inches diameter. I am not prepared to recommend any particular size to the notice of the Society, more especially as convenience must in many cases be consulted; but it seems probable, that the larger they are made the better, and for ordinary use they could be conveniently enough constructed of from 2 to 4 feet diameter, and of the form hereafter described.

I. The first error which may be noticed, is that resulting from an inaccurate discharge of the rain, hail, &c., actually in motion, arising from the contour of the adjacent ground or from the altitude and exposed position of the gauge itself.

All recent experiments have shewn that the results given by a rain gauge are intimately connected with its height above the surface of the ground, and have fully confirmed the somewhat unlooked for fact, that on the same place rain-gauges placed at different elevations above the ground indicate very different falls of rain.* The higher the instrument is placed above the ground, the less rain it collects. It is needless to enter on a consideration of this subject; but I may observe that Professor Stevelly's theory of an increase in the velocity of the wind as we ascend, appears to furnish the most satisfactory explanation of the phenomenon, the more so as from experiments which I have made, it seems to follow *that the wind is the great origin of error; that the stronger it is, the less rain is collected; and that in calm weather, the elevation, as well as form of instrument, are of little consequence.* But in whatever way this question may be settled, there is at least every inducement for placing the gauge on the surface of the ground.

Many opinions have been given as to the best locality for a gauge, and I suppose it is now generally admitted that an open plain is that which is most suitable. But as many who are most anxious to undertake these meteorological experiments have no champaign country to resort to, but are obliged to have recourse to small garden plots, encumbered with trees and shrubs, assistance in determining the most suitable position for the gauge may in such cases be derived from observing the fall of snow. This observation should not, however, be made when the wind is strong enough to disturb the snow after being deposited. The spot to be adopted for the gauge is of course that on which there is generally, with the prevalent winds, the same depth as has fallen in the neighbourhood. Observations on the depths of snow at irregularities in the ground, in valleys, and on the sides and tops of mountains, seem likely to give us more accurate notions regarding the atmospheric currents, on a knowledge of which so much depends. If, after falls of snow unaccompanied with wind strong enough to cause drift, a gauge has been found to have acted as a centre of attraction, or the reverse, it may safely be concluded that there must be something wrong either in its position or construction.

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* See Professor Phillips' Reports and Professor Forbes' contributions to *Meteorology.*

II. Having now considered the effects of different situations for gauges, the next error which may be noticed is only a probable one, and is supposed to arise, if it does exist at all, from the suspended water being collected into separate drops of rain instead of being uniformly spread over the surface, in consequence of which it is possible that drops may fall without the rim, instead of being split by it, and the receiver be thereby robbed of that fraction of any one of these drops which belongs to the area of the gauge. On the whole, however, this error may be considered a compensative one, and therefore entirely unimportant, at least in gauges of 2 or 3 feet in diameter.

III. The next error is peculiar to gauges placed on a level with the ground, and is occasioned by the current in passing from its regular surface (where it meets with an uniform resistance) to the hole occasioned by the mouth of the gauge. This error is considered by some to be the great objection to large areas. It is not indeed easy to estimate what may be the effect of enlarging the area in this instance, but there seems to be no reason why it should be greater *proportionally* with the large than with the small area. Perhaps the best way would be to submit this point to the *snow-test*, as a means of ascertaining whether a proportionally greater quantity enters a *large* than a small area.

IV. Evaporation of the rain, arising sometimes from a few drops having rested upon the rim or funnel of the gauge, and being there dissipated, is an error common to all gauges. It is by no means a very serious one, but it may certainly be augmented by increasing the size of the gauge, with the consequent diminished slope of funnel which would be necessary in order to reduce the cost of workmanship and general unwieldiness of the instrument. But it might perhaps be lessened by coating the funnel with coach varnish or with the fine seed of the Lycopodium or club-fern, so well known for its singular property of repelling water, in that respect resembling the bloom of fresh cabbage leaves, on which the drops of rain or dew being prevented from spreading, are always found to preserve their spherical form.

V. The next error, like one formerly noticed, is peculiar to gauges placed on a level with the ground, and is caused by the resilience or dispersion from the surrounding ground of the rain and hail, which are thus made to fall into the receiver. Should there be grass growing close to the edge of the gauge, as is generally the case, there is the farther risk of drops lodging among the blades of grass, and being afterwards blown into it.

Now a very few drops being blown into a small gauge would more materially affect the result than if the same number were blown into a larger gauge. By enlarging the orifice this error would increase simply as the diameter, while the quantity of rain that should be collected would be increased as the *square* of the diameter, and thus the great advantage of large over small areas is in this instance rendered evident.

VI. Resilience and dispersion, or the rebounding of the drops of rain, hail, &c. off or out of the gauge, arising from their impact against the rim or oblique sides of the funnel, is a serious evil in all forms of the instrument, and the loss occasioned in this way is, I suspect, greater than is generally believed. By enlarging the orifice the error would be diminished in the same high proportion as the last.

VII. The eddy occasioned by the rim catching the wind produces an error somewhat like the last in its extent, and it may be diminished in the same proportion by simply increasing the area. Sir John Leslie, in his article on Meteorology, in the Encyclopædia Britannica, has stated that there would be an advantage in having large areas, and describes the *rim-eddy* in the following words :—" We may suspect that the measure of the rain, hail, or snowy flakes received by the ombrometer is not exactly proportioned to the extent of surface which it presents; for while torrents pour down from the heavens, an eddy plays about the rim of the basin, deranging the regularity of the discharge."

This rim-eddy has been generally, and with justice, considered as a most formidable source of inaccuracy.

VIII. The last errors which may be noticed are due to the imperfections of form in the mouth of the gauge, errors in the graduation of the scale, or in other parts of the instrument, all of which will clearly be diminished by an increase in the area.

From the above considerations it appears that, by increasing the area, the tendency to evaporation, by no means a very serious evil, is augmented, and *perhaps* also the error caused by the passage of the wind from the uniform surface of the ground to the aperture of the gauge. On the other hand, by increasing the area, there is a diminution in the ratio of the squares of the diameters, of the errors arising from the rebounding and dispersing from the ground *into* the gauge, from the rebounding and dispersing from the rim and funnel *out of* the gauge, from the eddy caused by the rim and funnel; and finally, although not in the same high proportion, from imperfection in form of the receiving mouth or other parts of the instrument.

It is proper to mention, that in some experiments it has been found that the larger gauge gave a less quantity of rain than the smaller; from which circumstance many suppose that the small gauge is more correct than the large; but I think it only appears that in such cases the errors connected with the small gauge are in excess. It would not be proper at once to conclude of two gauges, that the one which gives the greater quantity of rain is the more correct, for it is quite possible that while one gives the correct, another may give too high a result; and as we do not possess the means of ascertaining which of the two instruments is the correct one, we must, in all cases, give the preference to the one whose *form and construction seem best fitted for the object to be attained.*

Having endeavoured to shew the superiority of large gauges over small, I shall now describe the form of instrument which appears to me to be best suited for the purpose. What I conceive to be its peculiarity is the *smallness* or rather the *want of a rim*, and the advantageous position of the funnel.

The mouth of the instrument which is recommended, is intended, as shewn in the diagram, fig. 1 (representing a gauge of the common size), to be sunk to a level with the ground (*h f g i*), which is generally considered the best position by those who have experimented on the subject. On the top of the funnel (*a b c*), which has a larger mouth than that of the receiving area (*d e*), there is placed a zone (*a d e c*) projecting inwards about six or nine inches, thus reducing the area of the receiving surface to the required dimensions. There is therefore exposed to the wind a very small rim (at *d* and *e*), while the shelving sides of the funnel come only as high as the bottom of the zone, and are therefore so far below the aperture of the gauge as to render it quite impossible for even hail to recoil in such a way as to escape out of it. To prevent rain from without being blown up the inclining side (as at *a d* or *c e*) of the zone, so as to pass over the edge into the gauge, as well as to fill up the space between the edge and the surrounding grass, a circular brush *f d e g*, about 3 inches wide, is placed so as to present its bristles to the impinging rain, which sinks among them instead of recoiling and being scattered about. The wind, in passing from the grass to the receiving-hole, will, of course, meet with no inequality, as the top of the bristles is on a level at once with the surrounding grass (*h f g i*) and the edge of the zone.

Fig. 1.

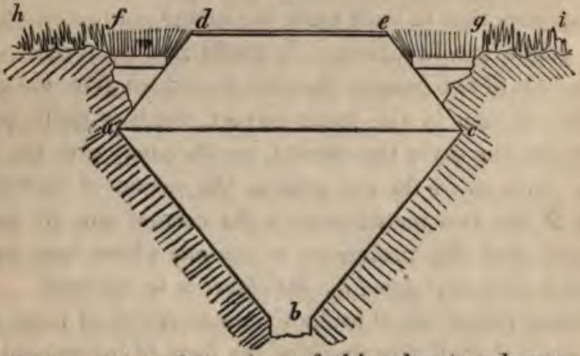
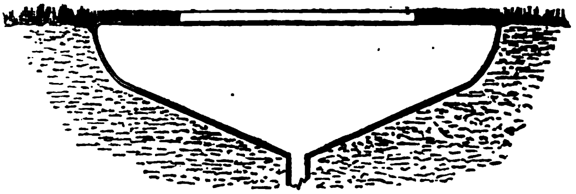


Fig. 2 shews the adaptation of this plan to a larger scale, in which case the brush is supplanted by bristles loosely arranged on the horizontal copper plate, and fixed either with wires or resin. A thinly cut sod or turf might probably an-
ery purpose quite as well as the bristles. It is advis-

able also that there should be a small rim $\frac{1}{4}$th or $_8$th inch high fixed to the horizontal plate.

FIG. 2.



The following very serious sources of error may be said to be nearly annihilated by the instrument above described, viz. the well known eddy resulting from the obstruction offered by the rim and funnel sides, resilience and dispersion of the rain and hail drops from the rim and funnel sides of the gauge, and similar resilience from the ground into the gauge.*

EDINBURGH, *January* 5. 1842.

Since the above was written, answers have been received from several of the light-keepers of the Northern Light-houses to queries sent them relative to the effects of high wind on gauges. Besides their other duties the keepers daily register the results of the rain-gauge, one of which was placed at each station by order of the Commissioners of the Northern Light-houses many years since, who justly considered that in this way they would be enabled, in the event of a shipwreck taking place, to ascertain more exactly the state of the weather, and at the same time render a valuable acquisition to science.

The answers alluded to are of considerable interest and value, consisting of the observations of forty intelligent men, on the effects of wind in connection with rain, but I shall only give a few of the results obtained.

The observations were confined to one month, and were made by forty-four keepers; but owing to the late gales, returns have been received from only forty of them, of whom *four* considered that the accuracy of the gauge was not affected by the wind, while *thirty-six* considered that it was.

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* I regret that I have not as yet been able practically to compare the results of this form of instrument with those commonly in use; but in a hail-*shower to which the gauge* was exposed, the surrounding bristles appeared *efficiently to answer the* purpose for which they were designed.

*Six* did not observe rain rebound out of the gauge, while *twelve* did; *six* did not observe hail rebound, while *twenty* did.

The following short extracts from some of these returns go far to shew the great tendency to error produced by the rim and funnel. The receivers of the instruments are elevated 4½ feet above the ground.

*Light-house, Inchkeith.*—" When the wind (says the Light-Keeper's Return) is high, no snow, and very little rain, goes into it."

*Buchanness.*—" When there is little or no wind, it is pretty near the truth—the more wind the farther from the truth."

*Kinnairdhead.*—" On the 21st (December 1841) it blew a gale, with snow and sleet, when very little, if any, went into it, and what went into it was soon blown out again, and there was no water in it next morning except a few drops."

*Tarbetness.*—" On the 21st there was snow, with strong breezes, and none rested in the gauge; it all went right over."

*Dunnet Head.*—" A part blows over that should fall in. On the 21st, in a gale with snow and hail, although we were three times at the gauge that day we did not see any snow or hail fall into the funnel; it blew wholly over."

*Pentland Skerries.*—" It was a gale while it lasted, and the hail bounded against the *funnel* nearly 3 inches, and was carried away with the wind, so that little remained of this shower."

*Start Point.*—" There is a great deal of hail, and some rain, *sparks out.*"

*Cape Wrath.*—(In a hail-shower) " Our observation was, that there was not one particle stopped in the gauge."

*Island Glass.*—" We have frequently seen hail rebound out when it strikes near the top or *mouth of the filler.*" * * *

" It does not give a correct result in gales of wind. For example, on the 13th instant, we had the strongest wind with rain of any this month. I on that forenoon watched the gauge from 11 till 12, until *perfectly drenched*, and in that time there were but two parts in the gauge. The wind moderated at 12, but the rain continued the same, and at 2 P. M. there were eleven parts in it."

*Lismore.*—" When it rains, with strong winds, there is not a fair proportion enters, and a great deal more hail rebounds *out than goes in.* In proof of our remarks there have been, in some days of this month, heavy showers, with strong breezes, at which times we could distinctly see the greater part carried past."

*Rhins of Islay.*—" During squalls with rain, there is only a part of the mouth that receives the rain, a portion being *quite dry*."* In hail-showers a good deal rebounds and is blown away.

*Mull of Kintyre.*—" In gales some of the rain rebounds, or is clean blown away."

*Corsewall.*—" While rain and hail fell during heavy winds, we observed both rain and hail rebound out again."

*Mull of Galloway.*—" The rain has been seen to fly out after falling into the receiver, and there is always a great deal more in it when moderate than when windy."

*Point of Ayre.*—" We have always thought that we got more with little than with strong winds."

*Calf of Man.*—" We observed during squally showers, and are of opinion that a great part is blown past the top of the gauge."

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## *Microscopical Researches on the Conformity of Structure and Growth in Animals and Plants.* By M. Schwann.†

M. Schwann's discoveries must be ranked among the most important accessions that have been made to physiology.‡ They enable us to establish a theory of organisation and its development, an object which has not previously been attained.

Valuable observations and discoveries in every branch of physiology have not been wanting, and some of these branches have reached a high degree of perfection. But in relation to the primary foundations on which the science must be reared, some of them have been imperfectly ascertained, and others cannot be said yet to exist; hence there is a complete want of connection among the insulated observations that have been made.

These bases or foundations do, however, exist; and M. Schwann in his work has now deduced from M. Schleiden's observations and his own, with as much perspicuity as penetration, the most general consequences which must go to form a theory of the organisation and growth of organised beings. We shall here present its principal features.

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* Part of the inside of the gauge being dry, is, however, no proof of inaccuracy.—T. S.

† An extract published by Prof. Müller in the *Archiv für Physiologie.*

‡ The work of Dr Schwann is entitled " *Mikroskopische Untersuchungen über die uebereinstimmung in der Struktur und dem Wachsthum der Thiere und Pflanzen.*" *Berlin, 1839.*

The most recent discoveries in the physiology of plants have demonstrated that the formation of the cellular tissue, the fibres, vessels, and spiral vessels, is reducible to that of cells. The origin of cells has been illustrated by an important discovery of M. Schleiden's) Müller's *Archiv*, 1838, p. 137). His starting point is what R. Brown calls the nucleus of the cell, which M. Schleiden names, for this reason, *cytoblast*. Its colour is most commonly yellow, its internal structure granular. Schleiden has even discovered in the interior of the cytoblast a corpuscle, the corpuscle of the nucleus, which appears sometimes in the form of a spot, sometimes under that of a hollow globule. These cytoblasts are formed freely in the interior of the cells among a mass of minute mucous globules; as soon as they have attained their full growth, a very small transparent vesicle rises on their surface, which is raised above the cytoblast like a watch-glass above the dial-plate.

In proportion as this cell enlarges, the cytoblast appears like a body enclosed in one of the walls of the young cell; its wall, on the inner side, is extremely thin, and, as it were, gelatinous; it can seldom be observed, and is soon absorbed with the cytoblast. The young cells are free in the mother cell, and assume a polyhedral form by pressing closely against each other. Now this is in what M. Schwann's discoveries essentially consist, regarding the cells of animals, and the primitive conformity of structure between animals and plants.

In the *Chorda dorsalis*, of which I have long since demonstrated (says M. J. Müller) the cellular structure, M. Schwann has found the nuclei of the cells; each cell of the *Chorda dorsalis*, of the *Pelobates fuscus*, has its lenticular cytoblast, applied against the interior wall of the cell; and we perceive in this small lenticular body one well defined spot, rarely two or three. In the interior of the cells of the *Chorda dorsalis*, young free cells are formed, as among plants.

The primitive structure of the cartilages is, according to M. Schwann, entirely cellular. At the extremity of the cartilages of the branchiostegial rays of fishes, we perceive small polyhedral cells, closely pressed upon each other, and whose walls are extremely thin. These cells have a round granular nucleus. Towards the centre of the ray, we notice the partitions of the cells thicken more and more. Looking still further towards the base of the ray, we cease to perceive the separation of the cells, and there remains only the appearance of a homogeneous substance, in which nothing is seen but small insulated cavities; around each cell, however, there is a ring indicating a trace of the true cellular wall; whence it follows that all the intermediate substance of the cellular cavities cannot be formed by the walls of the cells, but that the intercellular substance here essentially contributes to the formation of cartilage. This intercellular substance may be perceived even when the walls of the cells are *still touching each other*; they appear under the form of a triangle, situate *three contiguous* cells. The formation of the cartilage here depends, in part, upon the thickening of the walls of the cells, and partly on the intercellular substance. In the cartilages of the higher animals, the thickening of the walls of the cells has not been observed. The principal mass of the future cartilage appears to belong to the intercellular matter, which comprehends many generations of cartilaginous cells.

We may observe, in the branchial cartilages of the tadpole of *Pelobates fuscus*, a mode of developing the cells analogous to that of plants. Some of these cells contain simple nuclei, others contain smaller cells provided, in like manner, with a nucleus at their internal wall, and but little exceeding in thickness that of this nucleus; others, again, containing still larger cells than the latter; so that we may here find all the degrees of transition.

The mode of the formation of cartilage takes place, it would appear, without the participation of the vessels, in a manner analogous to the growth of plants.

With regard to the radiated corpuscles *(corpuscula radiata)* of the bones, which become apparent after ossification, the mode in which their canals are produced is not yet very clearly ascertained. According as we regard the cartilaginous corpuscles as cavities of the cells, whose walls, by becoming thickened and confounded with the cellular substance, would constitute the cartilage; or, according as we conisder these corpuscles as entire cells; while the intermediate substance of the cavities of the cells would be nothing else than the intercellular substance; these rays would be, according to Schwann, either small canals penetrating the cellular cavities in the thickened walls of the cells, or prolongations of the cells in the intercellular substance. In the first case, these minute canals might be compared with the porous canals in the cells of plants; in the second case, they would answer to the prolongations of these latter. M. Schwann regards this last opinion as most likely to be the correct one.

Besides the formation of young cells in the interior of those already existing, Schwann further distinguishes, in animals, the production of new cells outside of these, in a substance without structure, namely, the *cytoblastèma*. It is usually the nucleus that appears to be developed first, and then the cell around it.

In many animal tissues new cells appear on the outside of the cells already formed. In one instance the cytoblastema is interior, in another exterior.

Schwann's observations on the ovula, considered as a cell, have yielded the following results:—

The ovula, contained in the follicle of Graaf, is enclosed in a layer of granules, which are cells having a nucleus on their inner face, with one or two smaller nuclei (corpuscules du noyau). The cells originate in the liquid of the follicle of Graaf, as in a germinative matter. It is easy to comprehend how these cells, possessed of independent life, become developed when they arrive, along with the ovula, in the uterus, in order to form other tissues—the chorion, for example.

The ovula everywhere possesses an external membrane without structure, whether it be the chorion or the vitelline membrane; the ovula, *therefore, is always a cell.* The vitelline cell incloses the vitellus, and on its internal surface the germinating vesicle, with the germinative spot. If the germinative vesicle be a young cell, developed in the interior of the vitelline cell, it is probably the most essential element of the embryo; but if this vesicle is the nucleus of the vitelline cell, it loses its importance; and, judging from the analogy with the greater part of cellular nuclei, it should be at a later period totally absorbed, or rather still continue to exist for some time in a rudimentary state, without constituting any thing essential. The solution of this question cannot yet be attempted.

The vitelline globules of the egg of birds consist of cells of two sorts; the vitelline globules of the vitelline cavity, of the vitelline canal and of the nucleus, of the cicatricule (*Hahnentritt*) inclosing a still smaller globule. The other cells are larger and contain a granular matter; water makes them burst, and their contents are scattered on the outside. At first the young vitellus contains only the vitelline cavity with its cells; the true vitelline substance does not yet exist.

In the somewhat larger ovulas of the ovary, a layer of a yellow substance exists around the cells, itself surrounded by a bed of cellular matter. The yellow matter of the vitellus is then formed between a membranous external layer of cells and the internal cells. The lenticular germ is composed of globules of unequal size having granular contents. The germ of an egg which has been sat on four hours still contains these globules. In about eight hours the external layer appears, formed of very pale cells, without a nucleus, among which are found globules of the germinative membrane.

In an egg of sixteen hours, the serous leaflet is formed of cells, some of which contain a nucleus, and one or two minute nuclei (nucleolules). They contain, besides, a liquid and very small grains, having a molecular movement. These cells, the nucleus of which was observed by Valentin, soon assume the polyhedral form. The mucous leaflet is composed of cells, with a transparent liquid and grains. These cells, the outlines of which are usually of a deep colour like those of the cells of the vitelline cavity, lie loosely in the midst of an intercellular substance, which constitutes their cytoblastema. The first rudiments of the embryo are composed in part of small cells without a nucleus, in part of pale cellular nuclei, inclosing nucleolules.

M. Schwann divides the tissues of the animal organism, in respect to their original and cellular composition, into five classes. These are, 1*st*, independent and insulated cells, swimming in liquids, or simply situate near each other and moveable; 2*d*, independent (*selbstændig*) cells strongly adhering to each other so as to constitute a tissue; 3*d*, tissues in which the walls, but not the cavities of the cells, run into one another; 4*th*, fibrous cells, elongated in one or more directions, in order to form fascicles of fibres; 5*th*, cells in which the walls and the cavities are confounded with each other.

To the FIRST CLASS belong the corpuscles of the blood, the vesicular nature of which Schultz has demonstrated, whose nucleus continues resting agai[illegible] walls, when they are distended by water, as M. Schwann has remarked, and the contents of which form the red colouring matter. The lymphatic corpuscles, of a mucous and purulent nature, likewise belong to this class; all of them are cells with a nucleus.

The SECOND CLASS comprehends the corneous tissue, the pigment, and the crystalline tissue. The cells are independent, although their walls sometimes disappear.

1. *Epithelium.*—This is most frequently composed of rounded cells with a nucleus situated on their inner surface, and with one or two nucleolules. By their union they assume the polyhedral form. In the outer skin of the tadpole of the frog, Schwann has seen two nuclei in a cell, and one cell of the epithelium, with a nucleus in a large cell, which, according to Henle, does not take place among the mammifera. The cells of the epithelium may assume two other forms derived from the primitive globular form; they are either flattened, with the nucleus remaining in the centre of one of the surfaces, or else these flattened cells become elongated, as Henle has observed in regard to the epithelium of the vessels; young cells arise below the old, and diminish in height in proportion as they approach the surface (Henle), where the cells become elongated in a cylindrical form, as has been noticed in the intestinal mucus.

2. *Cells of the pigment.*—These have on their wall a cellular nucleus which determines the white spot seen in their centre. The nucleus is usually provided with one or two smaller nuclei (nucleolules). Some pigment cells are lengthened in different directions, in the form of hollow fibres, and compose stelliform cells.

3. *Nails.*—The nail of a male fœtus when near its birth, is composed of superimposed layers, which are less obvious on the lower side of the nail in proportion as we go nearer the root; the hinder half of this portion exhibits no layer, but consists of polyhedral cells, having distinct nuclei. The lamellæ of the nail, treated with acetic acid, divide into plates, in which we rarely distinguish a nucleus. The polyhedral cells of the root change by flattening into small plates. This flattening should render the nail thinner in front; but it is probable that a layer of epithelium is formed beneath which equalizes the thickness. The corneous tissue of claws is likewise composed, in the fœtus, of cells analogous to those of plants.

4. *Feathers.*—The medullary substance of feathers is composed of polyhedral cells, strengthened by a nucleus in the young feather. We first see a finely granular mass, in which are numerous small nuclei, some of them with a nucleolule: it is around these nuclei that the cells are formed. The latter are not developed in the mother cells, but in the neighbourhood of the organized matter of the feather which furnishes the cytoblastema. The fibres of the epidermis of the shaft originate in the cells of the epithelium, which are large and flat, and have a nucleus as well as nucleolules. These are long and flattened striæ; from each cell spring numerous fibres, then all trace of a cell disappears. The barbs are feathers in miniature, the shaft secondary to the structure of the principal shaft, the barbules likewise composed of epithelium cells in *juxta-position, and possessing* a nucleus.

5. *Crystalline lens.*—The fibres of the crystalline lens spring from cells, discovered by Werneck (See the Archives of Meyer Ahrens, 1838, 259). In the crystalline lens of the common fowl, after eight days incubation, no fibres are yet found, but only rounded, pale cells, some of which contain a nucleus. At a more advanced period, a few cells of larger dimensions enclose one or two smaller nuclei. In the embryo of a sow, about 3½″ long, the greater part of the fibres of the lens are already formed; a portion is still unfinished, and a great number of cells likewise exist awaiting their change. The completed fibres form a ball in the centre of the lentille. The fibres closest to each other are hollow elongations of the globules. At a later period, these fibres become garnished with dentated edges, as is seen in the dentated cells of plants.

Third Class. 1. *Cartilage.* (See page 22).

*Teeth.*—The enamel of a tooth not yet developed, when treated by a weak acid, preserves the same structure. The inner surface of the enamelling membrane which surrounds the crown of the tooth is formed of short fibres, with six faces, situated vertically, so that each fibre of the enamelling membrane corresponds to a fibre of enamel: they appear to consist of elongated cells. In a recent state they contain a nucleus with a nucleolule; above them are situated rounded cells, adhering to the enamelling membrame, which is no doubt the young state of the former. The enamelling fibres, properly so called, are probably separated from the enamelling membrane, in order that they may become soldered to the enamel already formed, and ossify along with it.

The proper substance of teeth originates from the fibres among which the dentary canals occur. The pulp of a tooth is composed at the surface of cylindrical cells, with a nucleus and nucleolules, and of round cells in the interior. Schwann thinks that the superficial fibres change into the substance of the tooth.

Fourth Class. 1. *Cellular tissue.*—The origin of the cellular tissue is the cytoblastema without structure. It produces in its interior rounded cells with a nucleus, which change into fibrous fusiform cells, enclosing a round or oval corpuscle, in which one or two dark points may be distinguished. The nucleus rests against the wall of the cell. These cells, by becoming narrow at the extremities, change into fibres. The points of the fusiform cells give rise to fibres which sometimes emit branches, and terminate by becoming transformed into a fascicle of extremely delicate fibres. The development takes place in the following manner: the division of the two principal fibres, which go off from the body of the cell in a bundle of smaller fibres, approaches more and more to that body; so that, at a later period, the latter becomes the point where the fibrous fascicle takes its departure; at a still later period, the fibrous fascicle springs directly from the nucleus; lastly, the cellular body divides wholly into fibres, and the nucleus rests naked on a fascicle of these fibres. *The* [illegible] *probably* hollow.

The fatty cells of the cellular tissue of the fœtus have likewise at first a very distinct cellular nucleus. If the membrame of the cell be thin, the nucleus rises above the drop of fat which encloses this membrane, a circumstance which does not take place when it is thick. The nucleus contains one or two nucleolules. The fatty cells of the skull of young Red-eyes (*Cyprinus erythrophthalmus*, L.) have sometimes two nuclei disposed in the same manner relatively to the membrane of the cell. There still exists in the cellular tissue of the fœtus a third species of cells; they are round and pale, containing a nucleus at their wall, with one or two nucleolules, not prolonged into fibres, and enclosing no fatty matter, but filled with small grains; this granular precipitate first shews itself in the vicinity of the nucleus. The cellular tissue of the fœtus yields no gelatine by boiling; the decoction contains a substance like pyine, with this difference, that the muddy precipitate, produced by hydrochloric acid, in the case of the latter, disappears by an excess of that acid.

2. *Tissue of tendons.*—The tendinous fibres derive their cells in the same manner as the fibres from the cellular tissue.

3. *Elastic tissue.*—The medial tunic of the arteries of the embryos of swine, 6″ long, contains many insulated cells, some of them round, others terminated by two or three prolongations, which are again divided. On the inner side we perceive the ordinary nucleus of the cell, with one or two smaller nuclei; we find, besides, an elastic tissue already formed. The ramified fibres of the elastic tissue, which are hollow, according to Purkinje, appear to come from their cells.

Fifth Class.—The following is the type of formation in this class: first, there exist rounded or cylindrical cells, or else these are stelliform. In the former case, the primitive cells are placed one after another, and soldered to each other at their point of contact. The partitions are then absorbed, so that the primitive cells are changed into secondary ones. The latter increase like simple cells. Such appears to be the mode of formation of the muscles and nerves.

In the second case, the stelliform cells meet at their prolongations where they unite; the walls are absorbed, whence a net-work of canals results.

1. *Muscles.*—According to Valentin's observations, the primitive muscular bundles are formed of small grains, placed one behind another, and soldered together; the primitive fibres only come from the division of the fascicle into smaller fibres. Schwann has observed in the cylinders of the primitive fascicles of a pig's fœtus, about 3½″ in length, a deeper border and an internal part, no doubt the cavity. In the clearest portion he could distinguish, besides some small granules, corpuscles of a larger size, oval and flattened; these nuclei often enclosed one or two other smaller nuclei. They are placed at more or less regular distances from each other, in the thickness of the cylinder and against its wall. In muscles of greater age, no trace of cavity could be seen, but the nuclei still remained a long while visible, and are situated in the thickness of the fibre,

although they often project beyond it in the form of small eminences. (According to the recent observations of Rosenthal, the nuclei of the muscles, even of an adult, are not wholly effaced.) The muscular substance, properly so called, of the cylinder, is produced by a secondary deposit in the interior of the canal. (The structureless case of small primitive muscular fascicles, which I observed long since in insects, appears to be, says Müller, the remains of the secondary membrane of the cells.)

(According to Valentin's last observations (*Archiv of Müller* 1840), there is to be seen in the blastema of the muscles, at first nuclei, with nucleolules, which soon surrounded themselves with very delicate cells. These cells become elongated and arranged in lines like threads of a conferva. On the walls of the secondary cellular membrane, which thicken more and more, longitudinal fibres are produced, and the walls of the cells are absorbed. The muscular fascicle then represents a tube, whose walls, proportionally thick, are formed of longitudinal threads, as transparent as glass, and the interior of which encloses the nuclei of the primitive cells.)

Each *nervous fibre* is a secondary cell, arising from the union of the primary cells provided with nuclei. Schwann believes that the white substance, which enters at a later period into the composition of the white nervous fibre which forms a continuous tube, the ribbon of Remack, is a secondary deposit of the internal surface of the cellular membrane. This white substance is in fact surrounded by a particular membrane without structure, like the primitive muscular fascicles. This membrane appears like a narrow transparent border, which is easily distinguished from the deeper contours of the white substance. This well-marked exterior definition of the boundaries prevents us, says Schwann, from regarding the membrane in question as composed of a cellular tissue. Schwann has sometimes seen in the nerves where the white substance is perfectly formed, a cellular nucleus disposed here and there in the edge of the exterior membrane. In the *grey nervous fibres*, the white substance is not developed. (According to Valentin, the cells of the *cerebral substance*, in young embryos, besides the granules they contain, and which will soon multiply, are surrounded by an enveloping mass. The commencing cell forms the nucleus, its nucleus the nucleolule, and the enveloping mass the fundamental mass of the ganglionic globule. When the cells have formed the nervous fibres, there are deposited on the surface of the latter the nuclei of the cells, cellular fibres, and fibres of cellular tissue.)

On the walls of the *capillary vessels* of the larvæ of frogs, we see the nuclei of cells at certain distances from each other. They are situated in the thickness of the wall, or on the inner face of the capillary vessels, on which they often form a prominent point. According to Schwann, the capillary vessels of the embryo are probably formed in the following manner: some of the cells of the germinative membrane change into *stelliform cells* by the prolongation of their parts. The elongations are ap- [illegible] *against* another, become soldered together, the walls are absorbed, and a network of canals is formed of a very unequal thickness. The contents of the primary cells, secondary or composite, is the sanguineous liquid. (According to Valentin, the internal membrane of the capillary vessels is produced from elongated or ramified cells. The exterior fibres, as well as the filiform epithelium, originate from the cellular fibres, which are formed and collected in masses on the exterior.)

Valentin distinguishes in the *formation of tissues* a secondary deposit of enveloping substance; it is observable in the ganglionic globules of the brain and nerves. The primary cell is produced along with its nucleus, and then itself performs the function of a nucleus, so that its nucleus becomes the nucleolule, and the primitive nucleolules become nucleolules of secondary power. Around the cell is disposed a mass of grains, united by a transparent matter, and surrounded by a simple cellular membrane. In the egg, we see new cells developed in the midst of the enveloping mass, which determine the formation of the vitelline globules, and other cells of still higher importance, and which, by their metamorphosis, have a direct influence on the development of the parts of the embryo. What takes place in the cellular formation of the first degree, the deposition of a heterogeneous mass around the nucleus, is reproduced in the second degree, in the ganglionic globules and in the egg. For other details, reference may be made to Valentin on the development of tissues (*R. Wagner, Physiologie*, 1339, 132.)

We shall give, in conclusion, and always according to J. Müller, a sketch of the principal results which Schwann has arrived at.

The most different elementary parts of animals and plants have a common mode of development; their origin is always a cell. We first see a substance presenting no structure, and which is found either in the interior of the cells, or between the cells already existing. It is in this substance that cells are formed, according to determinate laws, and these cells are developed in different ways in order to form the elementary parts of the organisms. In each tissue no new cells are formed, except in the points to which new nutritive elements penetrate; whence the difference between the tissues which contain vessels and those deprived of them. In the first, the nutritive fluid spreads in every direction; here the new cells appear in all the thickness of the tissue. In certain tissues without vessels, the nutritive fluid is brought only to the inferior, that is, the internal or adherent face, as takes place with an epidermis. In cartilages, when they are still without vessels, new cartilaginous cells appear only at the surface, and then arrange themselves in a circle.

The expression, increase by *juxtaposition*, is good when used to express the production of new cells, and not the growth of such as exist. The new cells of the epidermis appear only beneath the preceding.

In both these cases the cells grow by intro-susception.

Bones are found, up to a certain point, in a mixed state. The cartilage is at first without vessels, and the new cells are formed near its exterior surface. When vessels are developed in the medullary canals, the formation of a new cytoblast and new cells may take place in part at the surface of the bone, and in part around these small medullary canals. This explains the disposition of the cartilage in layers or plates concentric with the surface or with the small medullary canals.

The following, moreover, is the mode of the formation of cells: in the midst of a cytoblast, without structure or finely granular, we see, after some time, rounded corpuscles develope themselves; these are the nuclei around which the cells are formed. The nucleus of the cell is itself granular, and either filled or hollow. From the nucleus first springs the corpuscle of the nucleus; around the latter a layer of a finely granular substance is deposited; the nucleus increases. Around the nucleus the cell is at last formed, by the deposition of a layer of a substance distinct from the surrounding cytoblast. This layer is not very well defined at first. When once the membrane of the cell has acquired consistency, it becomes extended by the continual admission of new molecules between those already in their place; it is lengthened out, by this development, from the primitive nucleus, whence it follows that this nucleus remains fixed on a part of the internal surface of the walls of the cell. The formation of cells is nothing more, therefore, than a repetition of the mode of the formation of the nucleus, by means of which the latter is developed around its corpuscle; only the formative activity is greater in the development of the cell than in that of the nucleus. The membrane of the cell is chemically different in the different kinds of cells; and, in cells of the same nature, the chemical composition varies with age. According to Schleiden, the membrane of the youngest cells of plants is soluble in water; afterwards it is not so. The contents of the cells varies still more; fatty matter, pigment, &c. In the interior of a cell, which is at first as transparent as water, there may take place by degrees a granular precipitate commencing round the nucleus; on the other hand, the granular contents of a cell may dissolve insensibly.

It is easy to see that cells are small organs in which reside the power that presides over absorption and secretion. On all absorbing surfaces we find a layer of similar cells, which constitutes the epithelium; they surround villosities, and may be compared to the cells of the spongioles in the roots of plants. In the excretory canals of glands we likewise find, according to Henle and Purkinje, a layer of epithelium cells; the whole mass of the liver, and even the tissue of glands, without excretory canals (thymus, &c.), are likewise formed of cells inclosing a nucleus.

According to Schwann, all the cells exercise a remote chemical action (*metabolische*) on the cytoblast, which determines the secretions. The vessels conduct the liquid about to be modified; the cells which compose the canals of glands are the modifying elements.

With regard to the theory of cells, of which Schwann has now laid down the principles to serve as a basis to a theory of the vegetative functions of organized beings, I refer (says J. Müller), to the work itself.*

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* ***From*** Annales des Sciences Naturelles, January 1842, p. 1.

## *On the Comparative Evaporative Power of Coal and of Coke.* By Andrew Fyfe, M.D., F.R.S.E., F.R.S.S.A. Communicated by the Royal Scottish Society of Arts.*

In a paper read before the Society last Session, I stated that the evaporative power of different kinds of coal, would in practice be found to be on an average as the proportion of fixed carbon; for though the gaseous matter evolved from coal by application of heat is, or ought to be, consumed during its combustion, yet, as the gases during their evolution from the coal must absorb heat, they will by their consumption again supply to a greater or lesser extent that which they absorbed; hence has arisen the idea entertained by many, that coke will give forth as much heat by its combustion—in other words, will evaporate as much water as will the coal from which it is obtained. Now, though the evaporative power of coal which contains volatile inflammable matter seems to be in proportion to the fixed carbon, it by no means follows that coal, and the coke which it will afford, will in practice have the same evaporative power; and it was with the view of ascertaining whether or not this is the case that the experiments, the results of which I am now to lay before the Society, were undertaken.

The coal used was that from Tranent, and the coke was that from the same coal, prepared on a large scale in the common way for brewers and others in the neighbourhood. Previous to the trial, they were subjected to analysis to ascertain the proportion of volatile matter, fixed carbon, and ashes. The coal was as follows:—

| | Coal. |
|---|---|
| Moisture, | 13. |
| Gaseous matter evolved by heat, | 34.5 |
| Fixed Carbon, | 50.1 |
| Ashes, | 2.4 |
| | 100.0 |

This coal ought therefore to yield 52.5 per cent. of coke, provided the whole of the volatile matter were expelled; but this is seldom done when it is prepared on the large scale. Accordingly, when subjected to analysis, the coke was found to yield inflammable gas by exposure to heat. It must also be borne in mind that coke always absorbs moisture from the atmosphere. The composition of the coke, as found in the market, was—

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* Read before the Royal Scottish Society of Arts, March 28, 1842.

| | |
|---|---|
| Moisture, | 3.5 |
| Inflammable matter evolved by heat, | 6.5 |
| Fixed Carbon, | 81. |
| Ashes, | 9.0 |
| | 100.0 |

The gaseous inflammable matter was, therefore, in the coal and coke in the ratio of 34.5 to 6.5, while the fixed carbon was 50.1 in the former, and 81 in the latter. The proportion of ashes in the coke was greater than that afforded by the coal which yielded only 2.4 per cent. Accordingly, as 100 of coal gave 52.5 of coke, 100 of coke ought to have given 4.5 of ashes, whereas the quantity amounted to 9.

In conducting the experiments with the view of ascertaining the comparative evaporative power of these fuels, I had recourse to the small furnace and boiler, with which some of the trials stated in my former paper were made. The boiler was of the waggon shape, with a returning flue, and capable of holding 50 gallons. The fire surface of the furnace was 16 by 14 inches, and the surface of the boiler exposed to the fire was in all about 18 feet. The water supplied to the boiler was at the temperature of 42, and the quantity evaporated was, as formerly, ascertained by a glass gauge accurately graduated. The boiler being open, the evaporation was conducted under the natural pressure.

The trials were first made with coke, so as to have the surface of the boiler exposed to the fire as clean as possible. It is unnecessary to give the results of all the experiments. I select the following as the most satisfactory

*Tabular View of the Working.*

| Time. | Water evaporated in lbs. | Coke supplied about 7 lb. per hour. |
|---|---|---|
| 11 | ... | ... |
| 12 | 60 | ... |
| 1 | 50 | ... |
| 2 | 50 | ... |
| 3 | 50 | ... |
| 4 | 60 | ... |
| 5 | 50 | ... |
| 6 | 50 | ... |
| 7 hrs. | 370 lb. | 50 lb. |

In this trial, which lasted for seven hours, 50 lb. of coke were used, and 370 lb. of water were evaporated from 42, and $\frac{370}{50}$ = 7.4—accordingly, for each lb. of coke consumed, 7.4 lb. of water were driven off in steam.

In another trial, conducted with equal care and for the same time, the result was very nearly the same.

The coke used, it has been already stated, contained 81 per cent. of fixed carbon. Now, according to the experiments of Despretz, 1 lb. of carbon will evaporate 12.3 lb. of water from 32, and the number of degrees of heat in the steam beyond that point is 1136; the water which I employed was at 42, consequently the degrees of heat in the steam beyond it were only 1126; the coke should therefore have evaporated only 7.33 instead of 7.4; supposing the temperature of the water supplied to the boiler to have been at 32,

and as 100 : 12.3 : : 81 : 9.96,

accordingly, the coke ought to have evaporated 9.96, provided the whole of the carbon were consumed, and provided also the whole of the heat evolved by the combustion were taken up by the water; but the quantity was only 7.33, therefore there was a loss of about 26 per cent.

Similar trials were then made with the coal. The following is a tabular view of the most satisfactory :—

| Time. | Water evaporated in lb. | Coal about 8 lb. per hour. |
|---|---|---|
| 9 | ... | ... |
| 10 | 40 | ... |
| 11 | 40 | ... |
| 12 | 45 | ... |
| 1 | 40 | ... |
| 2 | 45 | ... |
| 3 | 40 | ... |
| 4 | 40 | ... |
| 5 | 40 | ... |
| 6 | 45 | ... |
| 7 | 40 | ... |
| 8 | 45 | ... |
| 11 hrs. | 460 lb. | 82 lb. |

In this trial, which lasted for 11 hours, the coal used amounted to 82 lb. and the water evaporated was 460 lb.; and $\frac{460}{82} = 5.61$; accordingly, for each pound of coal consumed, 5.61 of water were evaporated; but, as the water was at 42, the result would have been only 5.56, provided the temperature of the water were at 32.

In another trial the result was 5.8, or supposing the water at 32, it was 5.66.

From the results of the experiments which have now been stated, it is evident that the practical evaporative power of coke is by no means so great as that of the coal from which it is obtained; for, had it been so, a much greater amount of evaporation ought to have been procured. It has been stated that 1 lb. of coal evaporated in the furnace and boiler which I used, 5.66 lb. of water from 32. Now, this coal yielded 52.5 per cent. of coke; 1 lb. of the coke got from the same coal evaporated 7.33; consequently .525 would have given only 3.84.

It may be objected to these trials, that as the coal evaporated only 5.66 of water, the furnace was not well adapted for the consumption of fuel. It must be allowed that the loss of heat appears to be considerable, yet it was not much more than we frequently find in furnaces of steam-engines, with which it is considered a good result when 1 lb. of coal evaporates 6 lb. of water. Besides, the trials were made not with the view of ascertaining the actual amount of evaporation which could be obtained by the combustion of a given weight of fuel, but the comparative evaporative power, or, in other words, what can be done in practice; consequently, if there was a loss of heat in the one case, there would be the same, or nearly the same, in the other. Considering the experiments, the results of which have now been given, in this point of view, viz., that 100 lb. of coal evaporated 566 of water, and that this coal yielded 52.5 of coke,—then, had the evaporative power of the coke been the same as that of the coal from which it was got, 52.5 lb. of coke ought also to have given the same amount of evaporation. But, in the trials, 100 of the coke evaporated 733, and 52.5 would therefore have given only 384,—making a deficiency of 182 below that which the coal afforded.

I may here remark, that the result of the experiment with coal is another proof, in addition to those given, in confirmation of the conclusion which I deduced from the results of the trials stated in my former paper, viz., that the practical evaporative power of bituminous coal is in proportion to the fixed carbon; 1 lb. of carbon will evaporate 12.3 of water; now, the fixed carbon in the coal, used in the trials just stated, was 50 per cent.; the quantity of water evaporated by the combustion was 5.66; it ought from the proportion of fixed carbon to have been 6.16; the deficiency is only 0.5, thus making a very near approximation.

Supposing this position to be correct, it may very naturally be asked, Why does not coke also evaporate according to the fixed carbon which it contains? Now, to this a satisfactory answer can, I conceive, be given. In coal there is bituminous matter, which, before it is inflamed, must assume the gaseous form, and must, therefore, take up a part of the heat evolved by the combustion of the fuel previously on the fire, part of which it may again evolve by its own combustion, and thus supply that necessary for the draught; but, in coke there is very little of the gaseous inflammable matter; that in the coke I used was only 6.5 per cent. Now, though this would absorb very little heat during its volatilization, it would of course give forth little by its combustion; hence a part of that evolved by the fixed carbon may be necessary for maintaining the draft; for, let us suppose the coke to be pure carbon, we never could expect that in practice it would evaporate to the full extent of what it is calculated to do, for, in that case, no allowance whatever would be made for loss of heat to the walls of the furnace, and more particularly, for that, which it is well known always passes off with the gaseous products of combustion up the chimney.

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## *On some Geological and Physical Considerations connected with certain portions of the Glacier Theory of M. Agassiz.*

By Professor H. G. Bronn of Heidelberg. 50

The remains of fossil elephants seem to present an incongruity in the theory. As these remains are contained in the diluvial strata elevated by the Alps, the animals must have lived and perished along with their contemporaries at the period of the formation of these strata; and, as they occur in a state of good preservation in the Siberian ice and frozen soil, it must also be assumed, that these animals were still in existence and were suddenly annihilated and enveloped in ice at the time of the suddenly occurring cold of the ice period, therefore not till after the formation of the diluvium, which could not have been likewise caused by this cold.

With this subject we must here connect the questions,—if it be actually susceptible of proof, from these remains of elephants, that such a refrigeration of the temperature of Siberia took place at that time?—if, at the time when the elephants existed, it was actually so *much* warmer in that region?—and if the supposed cooling must have taken place so *suddenly?*

In the first place, the occurrence of living species of elephants in warmer regions is no proof that extinct species required as warm a climate. At the present day, we still find various species of the genera *Ursus*, *Canis*, *Cervus*, and *Bos*, distributed throughout every zone. Cuvier, also, in reference to the former climate of Siberia, directs attention to the fact, that the species of elephants enveloped in the Siberian ice had by no means a naked skin, but was provided with a covering of three kinds of hair, one under the other, and hence was more capable of inhabiting colder regions than are our living species. Even at the present day, among the Tungouses, the camel of the hot deserts is associated with the rein-deer of the northern icy regions as a domestic animal. It has, however, been said, on the other hand, that the incalculable number of elephants whose remains are buried in Siberia could not possibly have found sufficient nourishment there. But we must remember that these remains have been buried very gradually during the course of thousands of years, and therefore by no means indicate a very numerous race. In summer, moreover, the soil is by no means so unproductive as is generally supposed. Even where it is frozen to a depth of several fathoms, large trees take root in it, for in summer it thaws to a depth of from 2 to 16 feet. Around Jakutzk itself, where the mean temperature is —7½° C. (about +20° F.), and where the soil is frozen to a depth of several hundred feet, there is a considerable quantity of fruit, and the rearing of cattle flourishes; woods of larch cover the mountain acclivities to a height of 2400 feet above the level of the sea.[*] But Jakutzk, though it lies much to the south, is still nearly in the same isothermal line as those coasts of the Icy Sea which are so rich in remains of elephants. It will, however, be said, that elephants, at all events in winter, could find little nourishment in these regions; but the answer to this is, that, like so many other quadrupeds, they might have wandered to the south at that season. Like the bears of the Polar Sea, the land-bears of America, the squirrel (*Sciurus Carolinianus*) of America,[†] and like the lemmings of Scandinavia, which at the beginning of winter regularly migrate in armies of thousands, so might the elephants have done, for they are otherwise well known to be animals that perform extensive wanderings. But do not the excellent preservation of the ivory in the soil of Siberia, the envelopment of the elephants with their flesh and hair in the ice-blocks of the Polar Sea, prove at least that these animals must have been suddenly surprised by the cold, inasmuch as under other circumstances the portions of their body would have been subjected to decomposition? But the number of perfectly frozen individuals is but small compared with the whole amount. The bodies of these few, perhaps the last of their species, could easily have been transported from the localities, for them but partially habitable, by means of floods of impetuous rivers, to the Icy Sea. I will not, however, have recourse to this hypothesis, but will confine myself to the peculiar constitution of the country. Supposing that such animals were there destroyed by floods, by sinking in marshes, &c., during the summer season, and that they sank in these waters to the greatest depth to which the soil is thawed in the middle of summer, they would suffer but little by being kept at a temperature near the freezing point, under water which in a few weeks was again frozen. I may here notice that, in Great Britain and in North America, not only skeletons of species of deer with gigantic antlers, but also elephants, have been found in such a state of preservation and such a position as to shew plainly that the animals must have sunk in the marshes and moors. If the icy soil was thawed to a greater depth during a succeeding summer, the only consequence would be that they would sink deeper into it, and would thus be more protected from decomposition in the following seasons. Lastly, if, probably in consequence of the very phenomenon which caused their destruction, they were covered with accumulations of sand and mud, which elevated the surface of the soil,* the melting point would thus rise in the soil, and the height to which it always remained frozen would increase; the destroyed animals would thus be for ever protected against decomposition. Thus, then, it is precisely this, at first sight, remarkable fact, which least of all indicates a diminution of temperature, or a sudden and unusual cooling of the earth's surface.

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[*] See Erman, in his introduction to the "*Archiv für wissenschaftliche Kunde von Russland.*"

[†] Michaux, *Voyage à l'ouest des Monts Alleghanys*, Paris 1804, p. 189.

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* Thus, it appears from authors, that the soil of Siberia consists in many pla[illegible]ing layers of ice and frozen sand,—a circumstance which [illegible]ch an occurrence.

A sudden and pretty extensive cooling of the earth after the existence of the elephants is therefore by no means proved by the occurrence of frozen elephants in the Siberian ice, and is just as little required for the explanation of this phenomenon as for that of erratic blocks. We should, however, meet with much greater obstacles were we to inquire into the cause which could have produced such a refrigeration. From the theory of a gradual cooling of the earth from a red-hot condition to its present temperature, with which the hypothesis coincides in other respects, we can only, apart from local modifications, deduce a constant decrease of temperature, and there is also no other astronomical or physical cause by which an interrupted and intermitting diminution can be explained. Without our being able to assign such a cause, the hypothesis is not in any one point sufficient. It requires not only that the temperature of the earth should have fallen, but also that it should afterwards have been elevated; and not only is it necessary that this inexplicable occurrence should have taken place at the end of the tertiary period, but also that, after each of the five geological periods usually assumed at present, such a great sudden diminution of temperature should have destroyed all life, and that then the heat should have again increased, and awakened new beings, in order to continue at an equal height during the next period. In the work of Agassiz, we find only two attempts to establish this incorrect and unnecessary theory. On one occasion, he remarks generally (p. 306), " Nothing favours the idea that this diminution of temperature was gradual; on the contrary, whoever is accustomed to regard nature in a physiological point of view, will rather be inclined to assume that the temperature of the earth was lowered by successive steps, and was again somewhat elevated," &c. But the whole refrigeration theory, the whole foundation of our present geology, which the ice-period hypothesis itself recognises, is in favour of the gradual cooling; from it no other mode of cooling can be deduced, and the kind of argument used by our author on another occasion, " nothing favours the idea," may with truth be directed against his own supposition. At another place, p. 295, he says that it was the *elevation* of the Alps out of the immeasurable covering of ice "which again suddenly altered the relations of the climate of Switzerland, and gave rise to those frequent oscillations and variations in the ice-crust of Switzerland caused by the alternations of season and changes of weather." But if the elevation of the Alps could have had an influence on the climate, that can have been only local, and not one destructive of the icy envelope of the whole northern and temperate zone; and it can only have had a refrigerating influence and not a heating one, because we still perceive that thus Switzerland became an actual repository of eternal snow and ice, under all circumstances, in the midst of the temperate zone, and operated with a refrigerating influence on the surrounding countries; and therefore Charpentier* has correctly assumed for his theory that a former greater elevation of the Alps may have been the cause of the former greater extent of the glaciers,—a supposition which we find answered by Agassiz by the argument that "nothing favours the idea" (p. 281). There still remain for us, then, two investigations in relation to the theory of the ice-period: that as to the last-mentioned question, viz., if really there is nothing to support the notion of a former greater height of the chain of the Alps; and that as to the actual causes which are capable of explaining the alterations of climate since the formation of the diluvium.

If the Alps rose in a burning liquid condition, as is assumed by Agassiz, there can be no doubt that originally they actually had a greater height than they have at present; and this because every body expands by heat, and contracts by cold. I shall only adduce the observations of Professor Bischof, from which it appears that granite, in its passage from the liquid to the crystalline state, is contracted by 0.25 of its volume. At the time when the Alps were covered with a crust of ice, their surface must, it is true, have been completely cooled; but this did not prevent their interior being still in a red-hot state, and their nucleus being liquid; just as we find Etna and many other active volcanos covered with eternal snow. In this way, however, the largest portion of the contraction must previously have taken place, or at least the hardened outer crust could no longer follow completely the contraction of internal nucleus. But, as according to Fox, granite, from its red-hot condition down to the usual temperature, contracts by about 0.02, this would, in the case of Mont Blanc, amount to 300 feet for the portion projecting above the level of the sea, without taking into consideration that greater elevation of temperature and expansion of the portion of the vast crust of the earth on which the Alps rest, which must necessarily have been combined with this eruption, and which must, in a great measure, have existed after the cooling of the mountains, and without including the expansion of the merely elevated Neptunian strata. That, further, the sinking of a newly formed range of mountains, produced by the agency of fire, can be still measured within a short period, when that range is susceptible of being covered externally with snow, is proved by Boussingault's report on the Andes, in which chain the mountain of Quaguapichincha, near Quito, though now clear of snow on its summit, was covered with so much a hundred years ago that the French geometricians were interrupted in their labours; the volcano of Puracé also, near Popayan, according to the assertion of the inhabitants, has its snow line nearer the summit than formerly; and, according to the measurements of Boussingault, Quito, Popayan, Santa Fé de Bogota, and the Meierei of Antisana, are not so high now as they were found to be thirty years ago by Caldas and Humboldt. These portions of the Andes consist of trachytic rocks, which contract only by 18 instead of 25 per cent. We are, therefore, rather entitled to say, that nothing favours the supposition that the Alps have constantly possessed the same height above the level of the sea since their elevation.

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* *Jahrbuch*, 1837, p. 471.

Nevertheless, it is not my intention to deny all diminution of the temperature of the earth's surface, or even local alterations of temperature since the disappearance of the elephants. As, however, it was only in the last period of the earth that the temperate zone could be more and more distinguished from the hot, and the cold from the temperate, a degree of medium temperature was thus so unequally distributed over the surface of the earth, that such a temperature would not be perceptible in the hot zone, but little in the temperate, and most of all in the cold zone, where it would operate most powerfully in winter and at night, so that a degree of diminution of the mean temperature of the whole earth, distributed in the manner indicated, must have made the winter at the edge of the Polar zone several degrees colder. At all events, therefore, since the period of the disappearance of the elephants, the alterations in the temperature of the earth, taken altogether, have been limited to the increase of the cold of the winter and of the nights in higher latitudes, which had, as a consequence, an increase of snow and ice towards the Pole and on the mountains near it, and also an indirect action of this ice on the temperature of the region, and especially on the changeable nature of its summers.

If, however, these alterations of temperature should not be sufficient to explain the phenomena connected with the early glaciers, the intervening local alterations of temperature rendered possible by continental elevations and depressions, by marine and aerial currents, and by the local coolings of the crust of the earth, and also, according to all the indications afforded, the enormous length of the period of thousands of years which elapsed during which similar local phenomena could affect all parts of the temperate and cold zones, are more than enough to account for them, when we reflect that these local phenomena have even now the result, that in our hemisphere many such places which are separated from each other by from 20° to 25° of latitude, come under the same isothermal lines, and have a similar mean temperature, while, on the other hand, there are places lying in the same latitude which present a difference of mean temperature of from 10° to 12° cent. (18° to 21°.6 F.), nay, of even 17° cent. (30°.6 F.), and when further we remember, that even in the historical period, during the lapse of from 200 to 300 years, Greenland (*Grönland*, formerly *Grünland*) has been subjected to such mighty alterations, that it has now become almost uninhabitable. What consequences would be produced on the climate (supposing the heat of the sun to remain the same) by a permanent increase of the yearly fall of snow in Switzerland, caused by any geographical change? And if we think of the fact that the quantity of rain in the Alps northwards from the termination of the Adriatic Sea, exceeds several times that of any other part of Europe, it will be evident on what accidental circumstances such an increase may depend.*

In conclusion, it still remains for me to discuss a subject connected, in many respects, with this matter, viz. that of geological periods, of which five have lately been generally adopted. These I have in my Lethäa designated by the names Carboniferous, Saliferous, Oolitic, Cretaceous, and Molasse (*Kohlen,—Salz,—Oolith,—Kreide,*—and *Molasse-perioden.*) This *Period edifice*, the offspring of limited observations on contracted surfaces, already totters in all its supports; but, nevertheless, it is still necessary to sustain it and to bolster it up, as the erection of a new and better one is probably not so easy, and as the old one still affords us a welcome point of departure.

The refrigeration theory of the earth does not admit the assumption of an interrupted cooling by successive gradations, or even (as Agassiz supposes) an undulation of cooling of the whole earth by simultaneous elevations and depressions. Thus neither refrigeration nor any other given cause affords us the power of assuming sharp boundaries between different periods, or of adopting the consequences which are thus deduced, and especially in regard to their animals and plants. These undoubtedly became changed, but that in a continuous and gradual manner. We must, on the contrary, whatever theory of the formation of the crust of the earth we may follow, admit that the Neptunian strata, which we arrange into groups, formations, and periods, could not, individually, have been deposited simultaneously in an uninterrupted manner, and with similar mineralogical characters over the whole surface of the earth; that rather a number of local modifications in the formation of strata, caused by the relations subsisting at that time between the land and water, must have occurred, which were everywhere different; that there were also many strata here and there destroyed, and that, consequently, no petrographical or geological character available for the distinction or separation of groups of strata could extend to the whole surface of the earth. Neither, therefore, have universal well-defined divisions existed in the history of the formation of Neptunian products, nor have marks of such been left for us. This consequence, so very easily deducible from the hypothesis of the formation of rocks, was for a long time not drawn, at the period when appearances seemed against it; and because, in sciences of observation, facts have so frequently overturned conclusions much too hastily formed, and soon afterwards individuals have been so fortunate as to explain the contradictory facts as a necessary consequence of the same hypothesis by means of new kinds of reasoning. When, therefore, I now assert the necessity of giving up the sharp distinction of geological periods, this is not the consequence of a preconceived theoretical opinion, but because the proofs of the opinion are already in existence.

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* *See the Physikalischer Atlas* of Berghaus.

At no period was the whole surface of the earth entirely sea, and at no period was it entirely dry land or in a marshy state; but many parts have been often in both conditions for a time or alternately. When a portion was continuously covered by the sea for a period not too short, the deposits then formed, and their contents, would remain of a uniform character, or only gradually become changed. If, in connection with such a state of things, another portion in the vicinity was covered by the sea at the beginning and the end of the period, but was dry the intervening time, there would be an abrupt interruption between the two deposits; in short there would be the same distinction as between the earlier and later deposits of the first mentioned portion, but without the intermediate links. If, in the first region, a violent current destroyed a part of the already formed connecting links, a similar but even more distinct interruption would arise, because the remaining positive traces of the destruction would become blended together. Events such as the two last, have no doubt occurred on a very great scale over central Europe, and these have given rise to the adoption of the idea of the distinct separation of the strata of the crust of the earth into five periods; perhaps, also, in many cases, the concealment of the actually existing intermediate strata in less opened up depths of the earth has contributed to the belief. Extensive mountain *elevations likewise*, with a high inclination of strata, on which horizontal strata have been deposited, have also afforded proofs of boundaries. When, however, we become more accurately acquainted with such concealed portions of the interior of the earth, or with the more remote parts of the surface, these local blanks will more and more disappear, and others will occur where previously only uninterruptedly graduating series of strata were known.* Supposing that geology had been developed in another portion of the earth than central Europe, entirely different divisions into periods would have been the result, and with just as much incorrectness in relation to the whole. There is still, however, no doubt the distinction, that in the first period of the deposition of the neptunian rocks, the temperature was higher, and was therefore more uniform over the whole surface of the globe, so that the living beings on the earth and the remains derived from them might have a uniform character; and that at that time also the continents, probably less elevated and extensive, might have presented a less interrupted continuity than at present of the strata formed under the water.

In my *Lethäa Geognostica*, I have already given a list of such species of organisms as are common to different periods, and I shall not return to it now, as here I have more to do with the strata of rocks than with the species of petrifactions. While I willingly admit that, by a more minute examination, many of these species may be divided into two, or may turn out to be incorrectly determined; yet still the number of such common species has since been much increased, notwithstanding that much more careful determinations are now made, and De Verneuil and D'Archiac have prepared more extensive lists.

I. Regarding the boundaries between the present time and the molasse period, it is known that there are tertiary deposits which have 0.95, 0.90, 0.80, 0.50, 0.020 of their fossil species that are common to the living creation, and though some intermediate links are still awanting, yet these will yet be found. Here, therefore, we can hardly assume any real limit.

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* *Constant Prevost likewise expresses the same opinion.*

The observations made as to the remains of extinct animals occurring along with remains of man in original repositories, can hardly be said either to be confirmed or contradicted.

II. For a long time it appeared that there existed a very marked limit between the molasse and cretaceous periods, especially as there was to be seen near Paris a rich and perfect series of strata at the boundary of the two, where they seemed to be very distinctly separated in their deposition. On the other hand, Grateloup persists in the assertion that at Dax, some chalk fossils occur above the chalk in the tertiary strata, and he names in particular *Spatangus ornatus*, Defr., *Galerites excentricus*, Lamk., and *Galerites semiglobus*, Lamk.,* although Des Moulins† only enumerates the first in the two formations. The sandstone of Gosau has yielded, besides tertiary fossils, *Pecten quinquecostatus* and *Trigonia scabra*, and, according to Sedgwick and Murchison, also ten other chalk species. Lyell instances such mixtures in the Faxöe limestone on Faxöe. The green sand at Aix la Chapelle contains, according to Dumont's list, seven tertiary fossils out of twenty-eight species determined; according to Davreux, five out of thirty; and according to Hœninghaus, five out of twenty-eight, which D'Archiac has already included in his list. In the Crimea, according to Dubois, many fossil species of the older tertiary strata extend into the chalk, and that chiefly in a nummulite limestone and a hard marl. The same phenomenon is repeated in Egypt, where two characteristic species of the oldest beds of the *Calcaire grossier* (*Neritina perversa*, and the mould of a large *Cerithium*) occur in the chalk, while certain *Exogyræ* and a *Baculite* extend into the same nummulite limestone which was mentioned as containing so many tertiary fossils in the Crimea. We borrow this information from de Verneuil's memoir on the Crimea.

III. The boundary between the chalk and the oolite series has been very much effaced by the discovery and separation of the Hils-clay and Neocomian strata, as the lowest members of the chalk; the former is, according to Rœmer's recent investigations, essentially a portion of the chalk (*Speeton clay*), but contains in Hanover *Exogyra spiralis*, *Pecten lens*, *Terebratula perovalis*, *Serpula volubilis*, and *Cellepora orbiculata*, in common with the Coral-rag and Portland limestone; therefore a larger number of species than are common to the lower oolite and Oxford clay, &c.[*] Although Rœmer, in his new treatise on the chalk, does not enumerate these species (with the exception of the *Terebratula*), or forms new species for the individuals occurring in the Hils-clay, yet, on the other hand, according to Dubois, a much greater mixture of Jura and chalk species occurs in the same formation in the Crimea, where in the chalk, out of 49 species, 16 present themselves in the oolite. According to Fitton,[†] 15 species of the oolite occur in the chalk of the south-east of England, but, in regard to this, it is proper to remark, so far as these are derived from Fuller's-earth, that Rœmer[‡] considers Fuller's-earth as the equivalent of the Hils-clay or Speeton clay. In Yorkshire, Phillips finds that the Knapton and Speeton clays afford, out of 107 species, 99 from the chalk and 8 from the Kimmeridge clay, which latter is there awanting. Montmollin and Alexander Brongniart enumerate in the Neocomian strata of the Jura, the former 4 out of 14, and the latter 2, as belonging to the oolite. Miss Bennet mentions, that in Wiltshire, 5 species of the coral-rag occur also in the upper green-sand, but I do not trust entirely to the determinations. Other common species from various localities are to be found in the work of Goldfuss. Most of these species, and also some detected by himself, are ennumerated by D'Archiac in his list.

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* Grateloup's *Mémoire sur les Oursins fossiles*, p. 3.

† Des Moulins, *Etudes sur les Echinides*, p. 343, 393.

IV. The smallest number of common species has been found in the oolitic and trias, or saliferous series; and yet in Coburg and Würtemberg it is extremely difficult to determine with exactness the strata with which the one series begins and the other ends, as these beds pass into one another gradually and in a conformable position.

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[*] In the more recent works on the chalk, however, that *Exogyra spiralis* is regarded as a new species.

[†] Observations on the strata below the chalk and Oxford oolite in the south-east of England, 1836.

[‡] *Versteinerungen der Norddeutschen Kreide*, Hanover, 1841, p. 132.

V. Up to the present time the rocks of the trias and those of the coal or transition series seemed to be the most distinctly separated; a wide gap appeared to be opened up between them. This blank, however, has also very lately been filled up, since the appearance of the work by Münster and Wissmann on St Cassian. The formation of St Cassian, for so long a period a puzzle, has ended by becoming the explanation of another puzzle, the separation between the coal formation and the trias. It is a previously unknown link between the two periods, which, out of 422 species, contains 386 locally peculiar to it, and, among the 36 remaining species, has 22, which are one-half common with the transition rocks and the other with the trias; in favour of which double connection, the genera distinguished as belonging to the two give just as strong indications as the species. When, then, besides these there are some species partly analogous to, and partly identical with, those of more remote formations, viz. 11 with the lias, and 3 with the Jura, this scarcely amounts to a larger quota than occurs in other formations, but which I shall not enumerate, in order that I may not be too prolix. I would merely refer to the *Lethäa*, to Hisinger, Von Buch (on Terebratulæ), Adolphe Brongniart, D'Archiac, De Verneuil, &c.

Thus, then, the results of recent investigations have confirmed and justified what I asserted in 1832, that such cases must, in the mean while, be considered apart and isolated from the others, until more minute researches on the very spot should either teach us something different, or shew the cause of the mixture of fossils from different formations. If, however, we must admit such a mixture for the products of different periods, how much more will it be the case for the products of different formations or groups of formations of one period! When I read, three or four years ago, the assertion of Murchison and De Verneuil (which, however, was afterwards much limited by themselves), that all the fossil species of the Cambrian, the Silurian, the Devonian, and the mountain limestone systems were different from one another, I said, not only to my pupils, but in various published papers,* that these naturalists would the less deny a mixture of the fossils of the different systems the more they extended their views beyond the small country of England, and looked about them over a greater surface.

The result which has been obtained, that the species by no means became extinct at the end of each period, but partly lived during the succeeding one, also proves that these periods cannot have been separated in a very marked way; that such periods, to assume which has been so convenient for us, and will probably remain so for some time longer, *have never existed universally over the whole surface of the earth;* hence that a sudden refrigeration after a uniformly warm condition of the earth could not have destroyed simultaneously all life, in order to make way for a new race of beings, as is supposed by the ice-hypothesis, and that this universal cooling which is asserted, it is not known where or how, to have been brought upon the earth from time to time, also never existed. But against this "geological" view there now makes its appearance a new opponent that I have not yet mentioned, one which is intimately connected with the ice-hypothesis, and is the most alarming of all, viz. the hypothesis,† "that no character, that is, no distinguishing mark (of an organism) ought ever to be regarded as so trifling as to point to absolute identity; that it is not characters which mark out the species, but the combined relations to the external world in all the circumstances of life," (which, however, in fossil beings, are reduced solely to their stratographical distribution and their association with other species!). "Therefore the species cannot be recognized by resemblances, but only by their relations;" and without doubt, "in future it will be necessary to express the specific difference of organic remains by the circumstances of their occurrence, without it being possible to assign distinctions to them. And instead of being involved in boundless uncertainty, our science will emerge from its dry foundation to a state of rich blossom," that is, provided this blossom is not, as I hope, for ever frozen in the ancient universal ice! I have termed this opponent the most alarming of all, because, being in part a spectre, it has nothing corporeal about it upon which we can fasten, no foot that one can impede, and no head which one can strike off! Distinction of species without difference of characters: in what way can proof be adduced, and against what shall we bring proof? "By the circumstances of their occurrence?" In fossil species, that means neither more nor less than their stratographical distribution, which merely comes to this: Whoever wishes it, may declare that all the species of two periods are different, and whoever wishes it, may declare that all the species of two directly succeeding strata are different. It is true he cannot prove it, but neither can he be refuted, because, according to the hypothesis, no further proof is required.

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* *Jahrbuch, 1839, p. 735,* note; 1841, p. 97; 1841, p. 817, &c.

† *Jameson's Journal,* vol. xxxii. p. 97.

Let these glaciers move *chiefly* by the expansion of infiltrated water instead of their own weight,—grant that they have polished and furrowed many parts of the surface, that they have heaped up moraines into heights and hollows, nay, into whole mountains, where glaciers no longer occur, and that they prove a lower temperature to have existed at the time of their former extension,—yet all is not glacier action which the hypothesis declares to be such; least of all can it be concluded that the whole northern hemisphere was suddenly and simultaneously covered with a crust of ice as far as Mount Atlas, and that the whole earth was repeatedly and interruptedly suddenly cooled and then heated, in order alternately to freeze and awaken different races of beings. I cannot in any measure give my assent either to such doctrines, or to the opinion that species can be recognised without distinctions being ascertained, or established by characters which seem to have no stability, supposing even that whole periods of the earth's history lay between them. For, if the petrifactions presented to our investigation are to assist us in explaining the mysteries of the history of the globe, the value of these documents should not be destroyed by preconceived and arbitrary assumptions.*

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* *The above* remarks form part of a Memoir inserted in Leonhard and Jahrbuch for 1842.

## *On the Prevention of Smoke and Economy of Fuel by the use of Steam, in the Patent Process of Ivison.*

By Andrew Fyfe, M.D., F.R.S.E., F.R.S.S.A. Communicated by the Royal Scottish Society of Arts.*

In the papers on the Evaporative Power of Coal which I have already laid before the Society, I ventured to throw out the conjecture, that it depends on the proportion of fixed carbon which it contains, independent of the volatile inflammable matter; that, in fact, the greater the proportion of this ingredient, the greater would be the amount of available heat evolved during the combustion; and that I am correct in this opinion is still further confirmed by what was stated in the paper submitted to the Society on the 28th ult.

Thus, in the trials recorded in the first paper, with coal which contained 50.5 per cent. of fixed carbon, the practical evaporative power was 6.2. According to the fixed carbon, it ought to have been 6.2. In another trial in which the percentage of fixed carbon was 6.7, the evaporation amounted to 7.8, it ought to have been 8.0; and in a third it was 8.73, instead of 8.78. In the paper lately read to the Society, the result of trials with a different coal were given; it was 5.8; according to the proportion of fixed carbon, it should have been 6.1.

From these results I think that I am warranted in drawing the conclusion, that the practical evaporative power of a coal is just as the proportion of fixed carbon; that is, as the quantity of carbon which the coke from the coal contains. It is evident, however, that this remark applies only to bituminous coal, or to anthracite of inferior quality, and which, from its containing volatile inflammable matter, resembles other kinds of coal; for when a fuel is composed almost entirely of carbon, as is the case with the best anthracite, were it expected that the practical evaporative power would be equal to the amount of fixed carbon, which is occasionally 94 per cent., then this fuel would drive off nearly as much water in vapour as it is calculated can be done by carbon itself, provided the whole of the heat evolved by its combustion were taken up by the water. Now, it is well known that in practice this is never done, owing to the abstraction of part of the heat by the building, and from a considerable part passing up the chimney.

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* Read before *the Royal Scottish* Society of Arts, 25th April 1842.

Now, allowing that the available heat in practice is that evolved by the combustion of the carbon of the coke, and consequently that that which is evolved by the gaseous elements is lost, an important question arises, Can we by any means increase the evaporative power beyond that occasioned by the fixed carbon? in other words, Can we make available any of the heat which is, or which ought to be given forth by the volatile matter?

In carrying on the combustion of coal all the fixed carbon is consumed, with the exception of that which falls into the ash-pit; because, before any part of it escapes as a gaseous product, it must combine with oxygen; but this is not the case with the gaseous hydro-carbons. It is well known that a considerable part of these often escapes unconsumed as hydrocarbon, while another part, being decomposed, allows the combustion of the hydrogen, while a portion of the carbon is set free and not consumed; and hence smoke, more or less dense, according to the escape. This is occasioned by the want of due admission of air to the inflammable gases, at that part of the furnace where, when mixed with them, it would be exposed to the temperature requisite for their combustion. It is evident, then, that the more freely air is admitted, up to a certain extent, at the proper place, the more perfect will be the combustion of the gases. Hence the numerous contrivances for the admission of air, both cold and heated, at different parts of the furnace, by which smoke is so far prevented. Allowing, however, that this is done, and that, by these processes, the escape of any part of the gaseous elements unconsumed is also prevented, the question still remains to be answered, Whether the practical evaporative power is increased beyond that which is pointed out by the fixed carbon of the coal? Of course I now allude to bituminous coal in general use for steam-engine furnaces.

Numerous statements have been given of the amount of evaporation produced by the combustion of fuel in the most approved furnaces. In the experiments, the results of which I have given in the published papers already alluded to, the utmost from Scotch coal of good quality was 6.6 lb. of water from 32 for each pound of coal; and if we suppose the evaporative power of Scotch coal to be to that of English coal as 3 to 4, then, in the furnace that I used, the result would have been as 8.8 with a similar consumpt of fuel, which is nearly the same as that obtained by others. The highest products on record, with which I am acquainted, are those of Parkes, given in the Transactions of the Institution of Civil Engineers, vol. iii. part 1.; and by Henwood also in the same Transactions. In one trial by Mr Parkes, in which every precaution was used to prevent, as much as possible, the loss of heat, the evaporation from Newcastle coal was 10.32 from 212, which, allowing the degrees of heat in steam beyond 32 to be 1136, as stated by Despretz, would be reduced to 8.68 from 32. In my trials with Scotch coal it was 6.6; and keeping in view the comparative strength of English and Scotch coal, the results very nearly agree. In one of the trials recorded by Henwood, 100 lb. of Newcastle coal evaporated 16.95 cubic feet of water from 94, which gives 10.54 lb. for each pound of fuel, and reducing this to 32, the result is 9.96.

The quantity stated in the above trials is far short of what it is said coal ought to evaporate, whether we calculate this from the quantity of oxygen which it requires for complete combustion, or from the known composition of the coal.

In the paper on the Heating Power of Coal-gas, published in the Transactions of the Society for 1840, I have stated that it required the consumpt of about 16 feet of gas to boil off 1 gallon of water from 32, consequently it would require 1.6 to boil off 1 lb., *i. e.* 7000 grains. Now 1 of gas requires on an average about 1.8 of its bulk of oxygen for combustion, supposing the gas to be about the sp. gr. 570. 1 foot of oxygen weighs 587 grs., or nearly so, and 587 × 1.8 = 1057, consequently the oxygen necessary for the consumption of 1 foot of coal-gas, of the quality stated, will weigh about 1057 grs.

According to Despretz, when 1 of oxygen enters into union with other substances, heat is given forth sufficient to boil off 4.6 of water from 32. Now 1057 × 4.6 = 4862, which multiplied by 1.6 = 7779. In my trials the consumpt of 1.6 of gas boiled off 7000, and consequently there was, by this mode of calculation, a loss of about one-tenth of the evaporative power in my trials.

It is generally allowed that 112 lb. of coal will yield 500 feet of gas, consequently 1 lb. will yield 1.46, or very nearly 4½ feet. It has been shewn that in my trials 1.6 of gas evaporated 7000 of water, and according to the consumpt of oxygen, it should evaporate 7779, and as 1.6 : 7779 :: 4.6 : 22,362, and 22,362 ÷ 7000 = 3.19. Accordingly the gas from each pound of coal should, by its combustion, and provided there were no loss of the heat evolved, evaporate 3.19 of water from 32.

The average quantity of fixed carbon in Scotch coal amounts to 50 per cent., consequently it ought to boil off 6.15 of water from 32; now adding this to the 3.19 formerly mentioned, 1 lb. of coal should evaporate 9.34, provided the whole of the fixed carbon, and of the gas given off from it, as in the manufacture of coal-gas, were completely consumed; but this is short of what a pound of coal ought to do, supposing the whole of its inflammable ingredients were consumed, and provided also the whole of the heat evolved were taken up by the water.

In the table given by Mr Richardson, Lond. Phil. Mag., 1838, the average quantity of oxygen necessary for the combustion of 1 of Scotch coal is 2.47, which would make 1 lb. evaporate 11.3 of water. Now, deducting 9.34 from this gives a deficiency of 1.96, which must be accounted for by the formation of the tar and volatile oil, also produced in the manufacture of coal-gas, by the elements entering into a new state of combination, and which, if burned along with the fixed carbon and gas, should make up the evaporative power to 11.3, or thereabouts. If we deduct the 6.15, representing the evaporative power of the fixed carbon, from the total amount, the deficiency is 5.15. Now, allowing that, by the proper construction of the furnace, and by the due admission of air, so as to prevent the escape of any part of the volatile inflammable matter unconsumed, the evaporative power of bituminous coal, when burned by these means, does not go beyond that which the fixed carbon of the coal will afford, an important question flows from what has been stated as the results from the foregoing calculations. Can the evaporative power be increased beyond that of the fixed carbon; in other words can the gaseous products be also so consumed, that the heat evolved by their combustion, will also cause the evaporation of water, and thus increase the total amount of evaporation? The process which I am now to describe is one by which that object can be obtained. It is a modification of that to which I formerly drew the attention of the Society, and an account of which is published in the Transactions for 1838. I then shewed that by propelling steam *up through the fuel*, when in a state of combustion, the evaporative power is increased. I intend now to illustrate another remarkable circumstance accompanying the use of steam. When, instead of being introduced from below so as to make it pass up through the fuel, it is projected *above* it, a similar result is obtained as to increase in evaporative power, and, in addition to this, there is also the very desirable object, the total prevention of smoke. This constitutes the process lately patented by Mr Ivison of the Castle Silk-Mills of this place. From the numerous opportunities I have had of conducting experiments with it, and, from the very interesting results which I have obtained, I have thought it advisable to lay an account of them before the Society. From the time occupied in carrying them on, they are, I conceive, valuable as giving results from trials on a large scale; and from the minute attention bestowed on them, I may be permitted to state, that the most implicit confidence may be placed in them. Though I have witnessed many trials, yet my remarks will be confined exclusively to those conducted under my own superintendence, having been present during the whole of the time, and having myself assisted at the weighing of the fuel, and in measuring the supply of water to the boiler, so as to secure accuracy in the results.

The furnace with which the trials were made, was that originally at the Castle Silk-Mills. The boiler, which was cylindrical, with egg-shaped ends, was 18 feet long, and 3½ in diameter, with the flues of the furnace passing around it. It stood in an open shed, with 2½ feet of its upper surface to the length of 17½ feet exposed. The steam-pipe conveying the steam to the engine was 2½ inches in diameter, and lapped with a single ply of rope. The furnace was of the ordinary construction, the bars being 5 feet 2 inches in length, and in all 2 feet in breadth. The height of the water in the boiler was ascertained by a two-way cock, the distance between the extremities of the pipes being 3 inches. The water supplied to the boiler was taken from a tank, through which the waste steam of the engine was generally passed, and by which the temperature was raised, sometimes to 110 at other times to 180. It was forced in the usual way into the boiler by a pump worked by the engine. The coal was generally supplied to the fire at regular intervals; and to ascertain the quantity actually used, the fire was brought to the same state at the commencement and termination of the experiment.

The steam apparatus for the consumption of smoke consisted of a tube of ½-inch internal diameter taken from the upper part of the boiler, and conveyed into the interior of the furnace, where it terminated by a fan-shaped distributor, by which the steam was projected into the upper part of the furnace, occupied by the flame and gaseous products of combustion, and consequently *over and above the fuel.* In using the steam distributor, air is admitted either at the door, or by some other means, so as to bring it near the distributor. In the trials which I carried on, it was admitted by apertures in the door, or by keeping the door a little open. A stop-cock is placed on the tube connected with the distributor, to regulate the supply of steam; the necessary quantity being known by the effect produced on the smoke. When the steam is thus admitted, the part of the furnace occupied by the flame and gaseous products of combustion, and frequently rendered obscure by the smoke from part of the volatile inflammable ingredients not being consumed, instantly presents a different appearance; the smoke disappears; the flame becomes more brilliant, and the heat seems to be more intense; at the same time, if smoke was escaping at the top of the chimney it very soon disappears; the time depending on the height of the stalk and on the draft; and as long as the steam continues to be projected from the distributor, little or no smoke is visible. I conceive it is unnecessary to say more on this valuable part of the process; it has been witnessed by many, who can bear testimony to the efficacy of steam when used in this way as a complete preventive of smoke from furnaces however large.

I now proceed to the more important part of the process, I mean the effect on the evaporative power of the coal consumed. The prevention of smoke, and the consequent absence of soot on the sides of the boiler, would naturally lead to the supposition that a greater amount of evaporation would be produced, not only because that part of the fuel which otherwise escapes as smoke is consumed, but also owing to the absence of soot on the boiler, the transmission of heat would be more rapid, and consequently being more quickly taken up, less of it would be lost by passing up the chimney. These causes must operate to a certain extent, in every instance where smoke is prevented; but we shall find that the increase in evaporation is far beyond what could be accomplished by these alone, as the result of the trials which I am now to state will shew. I consider it unnecessary to state minutely the details of the numerous trials I have conducted with this process. I will first give the details of one which I select, because during its performance the most rigorous attention was paid to every thing that could affect the working, and it will therefore shew how the trials were in general conducted.

| TIME. Began at 6 A.M. | Fuel in lb. | Water supplied to Boiler in lb. | Pressure in Boiler. | Strokes of Engine per min. | Temp. of Water supplied. | REMARKS. Clear frosty day. Bar. 29.8. Wind west—rather strong. |
|---|---|---|---|---|---|---|
| 6.15 | 112 | 280 | 35 | 48 | 160 | |
| .45 | 112 | 480 | 28 | 44 | 163 | Boiler just blowing off. |
| 7.45 | 112 | 700 | 29 | 38 | 172 | Thermometer in chimney, 3 feet above entrance of flue, 300. |
| 9 | . . | 1300 | 26 | 45 | | |
| 9.45 | 112 | . . | . . | . . | . . | Stopped work for ¾ hour. Boiler rather fuller than at 6 A.M. |
| 10.45 | 112 | 1240 | 36 | 45 | | |
| 11.45 | 112 | 1100 | 30 | 42 | 164 | Lead not melted in chimney. |
| 1 | 112 | 920 | 27 | 44 | 170 | Lead melted in chimney; Zinc not. |
| 2 | . . | 540 | 25 | 44 | 180 | |
| 2.45 | 112 | . . | . . | . . | . . | Stopped for ¾ hour. |
| 4.15 | 112 | 1700 | 29 | 49 | | |
| 6 | 56 | 1940 | 28 | 36 | 177 | Lead melted in chimney; Zinc not. |
| 7 | . . | 1240 | 26 | 38 | 174 | |
| | 1064 | 11,440 | 29 | 43 | Average, 170 | |

The above table shews that, by the consumpt of each lb. of coal, 10.75 lb. of water were evaporated from 170. Numerous other trials were made in the same way, with other kinds of Scotch coal, sometimes for the same, sometimes for a shorter period, and with a pressure nearly the same, but occasionally lower. The following are the results:—

| Time, in hours. | Fuel, in lb. | Temperature of Water supplied to Boiler. | Water evaporated, in lb. | Water evaporated, in lb., for each lb. of Coal. |
|---|---|---|---|---|
| 7. | 448 | 60 | 4210 | 9.37 |
| 5.15 | 336 | 60 | 3860 | 11.48 |
| 3.30 | 200 | 60 | 2520 | 12.6 |
| 5. | 336 | 60 | 3920 | 11.66 |
| 11.30 | 1064 | 170 | 11440 | 10.75 |
| 8. | 756 | 130 | 7760 | 10.5 |
| 10.20 | 672 | 143 | 8360 | 12.44 |
| 11. | 588 | 110 | 8400 | 14.29 |
| 9. | 784 | 128 | 8080 | 10.3 |
| 5. | 392 | 130 | 5040 | 12.88 |
| | 5576 | | 63590 | 11.62 |

The average of the above ten results, which includes that given in the preceding table, is 11.62, from the average temperature of 105; and supposing the water to have been at 32, then the average result would be 10.87. Or it may be viewed in another way. In all the trials, 5576 lb. of coal were used, and 63,590 lb. of water were evaporated, and 63590 ÷ 5576 = 11.4, and reducing this from the temperature of 105 to 32, the result would be 10.66, and again 10.66 + 10.87 ÷ 2 = 10.76; a result which exceeds considerably the highest that has, so far as I know, been put on record—I mean that of Henwood, which was 9.94, procured by English caking coal, the evaporative power of which, compared to that of Scotch coal, is allowed to be as about 4 to 3.

With the view of ascertaining the power of the furnace and boiler, without the use of the steam apparatus, similar trials were made with the same coals, due attention being paid to the different circumstances already stated. On an average, I found that for the consumption of each lb. of coal, the evaporation amounted to 6.66, and reducing the water to 32, as before, the result would have been 6.17.

These results were still farther verified by noting the consumpt of fuel, during different periods of the day when the furnace was in use, with and without the steam apparatus. The following are the results:—

| WITHOUT THE STEAM. | | | WITH THE STEAM. | | |
|---|---|---|---|---|---|
| Time. | Period of Day. | Pounds of Coal used. | Time. | Period of Day. | Pounds of Coal used. |
| H. M. | | | H. M. | | |
| 5 15 | A.M. | 812 | 5 15 | P.M. | 560 |
| 5 15 | P.M. | 812 | 5 15 | A.M. | 440 |
| 5 15 | P.M. | 812 | 5 15 | A.M. | 612 |
| | | 2436 | | | 1612 |

During the periods stated in the above table, the engine connected with the boiler was all the time doing the same work; and consequently we have so far an indication of the economy of fuel by the use of the steam. When the furnace was in action without the admission of steam, the fuel consumed amounted to 812 lb. in 5¼ hours; and when steam was admitted, the consumpt, for the same time, amounted on an average to 537 lb. Now, as the same work was done by the engine with the consumpt of 537 lb. of coal when the steam was admitted, as was done by 812 lb. without it, the saving by these trials amounted to 34 per cent.; for, as $812 : 537 :: 100 : 66$, and $100 - 66 = 34$.

Considering the amount of evaporation obtained by the application of steam as now described, and keeping in view the results of the experiments which I have given, and also those recorded by others, it is evident that the evaporative power of coal is by this means greatly increased; but this is done by an expenditure of part of the steam generated in the boiler; it therefore still remains to be proved what the actual amount of saving is, after deducting the loss occasioned by the transmission of steam into the furnace. Different processes were followed, with the view of ascertaining the quantity of steam projected into the furnace. One of these was by comparing the area of the apertures in the distributor with that of the steam-pipe leading to the engine; but this was found liable to fallacy, owing to the difference in the escape of steam, which is not always in proportion to the area of the tubes or apertures through which it is transmitted, and owing also to the escape through the safety-valve. Another method was, to connect a tube with that leading to the furnace, and convey it through a refrigeratory, so as to cause the condensation of the steam, which, when condensed, was collected, and its weight ascertained. When this was in use, the supply of steam from the boiler to the furnace was cut off by means of a stop-cock, and the steam was then allowed to flow through the tube in the refrigeratory—the stop-cock by which the supply to the furnace was regulated being kept at the same degree as formerly. By this mode I found that the quantity of condensed steam varied considerably,—indeed, so much so, that though several trials were made, the results were so discordant, that I could place no reliance in them. This was owing most likely to the flow of steam into the furnace, through the minute apertures of the distributor at one time, and at another through the tube in the refrigeratory, being different, from the different conditions under which it was escaping, though the area of the aperture of the stop-cock was in all the trials the same. I had therefore recourse to another method. Instead of taking the steam from the boiler attached to the furnace, I had another smaller one erected near it, furnished with its own fire, and from which the pipe terminated by the distributor was taken, and conveyed into the large furnace. This small additional boiler was worked under a pressure of 6 lb.; and to avoid any error from the escape of steam by the safety-valve, the superfluous steam which should have escaped through it was conveyed by a pipe into water, where it was condensed and measured. Deducting the quantity obtained from it from the total quantity given off from the boiler, which was known by measuring the loss sustained by the boiler, the quantity projected through the distributor was ascertained. The average of several trials performed in this way, gave 4 lb. of steam for each 100 lb. evaporated from the boiler connected with the furnace and engine—that is, 4 per cent. Now, supposing this were taken from the boiler with which the evaporative power of the fuel was tried, when subject to the action of the steam distributor, we must deduct it from that discharged from the boiler, and we shall then have the available power in practice, and consequently we shall be able to ascertain whether, by this process, there is an increased amount of evaporation, and if so, what is the saving in fuel.

It has been already stated, that the average amount of evaporation with the furnace subject to the influence of the steam-distributor, was 10.76, presuming the water to be supplied to the boiler at 32. Now, deducting 4 per cent. from this, the residue is 10.33. With the same fuel, and without the steam-distributor, the evaporation was only 6.17—thus giving an excess of 4.16, or a little beyond one-half more, by the same quantity of fuel; consequently, to produce the same amount of evaporation by the common process, and by the use of the steam-distributor, the results shew a saving of fuel amounting to very nearly 40 per cent.

In giving the above as the increased amount of evaporation, and consequent saving in fuel, by the use of steam, I wish it to be understood, that the remark applies solely to the results of the trials above recorded. It is not to be expected that the same is to be got in all cases; indeed, it has been the reverse, for I must own, that in many cases, in which the distributor has been used, there has been no saving in fuel, nay, in several there has actually been a waste; more coal having been consumed in the same time to keep up the same amount of evaporation. Now, as it must be admitted, that what has been done with one furnace, should, under proper management, be done with others, it becomes of importance to ascertain what is the cause of failure in these particular cases. In order to discover this, it is necessary to attend to the different circumstances accompanying the action of the steam; and first, after the furnace is brought into good condition, from the moment that the steam is projected into the furnace, the passage of air into the ash-pit, and from it up through the fuel, almost entirely ceases, provided air be admitted by the door; indeed the combustion may be carried on with the ash-pit quite closed, and hence the absolute necessity of admitting air by the furnace-door, or otherwise, so that it shall be brought into contact with the fuel, and also with the gaseous inflammable matter evolved from it. The want of due attention to this circumstance has frequently caused a failure, not only as to the economy of fuel, but also as to the prevention of smoke; the deficiency in the supply of air causing the gaseous inflammables to escape, without being entirely consumed. Again, by the admission of steam, the draft is very much increased; indeed so much so, that where the chimney is tall, and consequently the draft originally is great, it becomes so greatly increased, that the heat generated by the combustion is hurried off, and time is not given for the water to take it up. It may naturally be supposed, that by lowering the damper, as is usually done, the excessive draft would be checked; but this is not the case, the position of the damper having little or no effect in altering the draft. I had recourse to several methods to obviate this disadvantage. The lowering of the chimney no doubt would, to a certain extent, accomplish it, but this is not attainable. To shew, however, to what extent the chimney may be lowered, I may here mention, that I had an opening made in it a little above where the flues entered it, and then passed an iron plate across, so as to prevent entirely the passage of the gaseous products upwards; they were of course forced out at the opening, and in this way I have again and again worked the furnace, and during the whole of the time the combustion and consequent evaporation went on as before, as was shewn by the engine continuing to do its work, though there was no chimney in use. The method which I have found to answer best in checking the draft, is cooling the products in the chimney; and the first means of doing this that occurred to me, was to make an opening near the base of the chimney, and admit cold air more or less freely, according to circumstances. In the first case in which I had recourse to this, and in which I could not succeed in keeping up steam, when the distributor was in use, from the moment that the cold air was admitted, the steam was easily kept up, and with the consumpt of a smaller quantity of fuel than before. There is still another circumstance to be attended to; I mean the mode of firing. It is evident, that if the steam acts in any peculiar way in causing the combustion of the gaseous materials of the coal, the coal ought to be so thrown on the fire, that these materials should be evolved as near to the distributor as possible; hence the necessity of pushing forward the ignited fuel, and then throwing the fresh coal as nearly as possible under the distributor, that the gaseous matter discharged by the gradual charring, may be brought under the action of the steam and air by which it is to be consumed. When this is not done, it is not to be expected that the smoke will be prevented, far less that there shall be a saving of fuel.

With proper attention to all of these circumstances, I mean the mode of firing, the due admission of air above the fuel, and the diminution of the draft, I believe that in most cases the process will be accompanied with the desired results. I say in most, for no doubt cases may occur, in which, chiefly from the difficulty of regulating the draft, there may be no saving, but, *on the contrary*, an increased expenditure.

With regard to the elastic force of the steam required for the distributor, I may here state that I have used it at pressures varying from 3 lb. to 35 lb., and invariably with the same effect as to the prevention of smoke. How far the difference in the elastic force of the steam projected into the furnace affects the amount of economy of fuel I cannot say, as all the trials made with the view of ascertaining this were with high-pressure steam.

It has been supposed by some, that however beneficial this process may be as to the prevention of smoke and economy of fuel, it is liable to serious objections from the destruction of the fire-bars, owing to the want of air passing up between them, and also from the injury occasioned to the boiler by the action of the steam from the distributor. As to the former, suppose it were to happen, it would be more than counterbalanced by the saving of fuel; but, so far as I have seen, the bars are not more acted on than in other cases; indeed, I would say less so; for, as there is very little or no air passing up from the ash-pit, the heat evolved at the bars is by no means so great there as it is when air is rushing up through the fuel. As to the boiler, the only one I have minutely examined is that with which I conducted the trials, the results of which I have given. After being in use for about eighteen months, during which I was from time to time experimenting with it, I had it examined by experienced engineers, who all certified that it had not sustained the slightest injury.

## *On producing the effect of Fog in a Portable Diorama, constructed by* GEORGE TAIT, Esq., Advocate. Communicated by the Royal Scottish Society of Arts.*

IN a description of a portable diorama which I exhibited to the Royal Scottish Society of Arts on 22d November last, I stated that the effect of fog may be represented by painting the objects intended to be affected by it, on a second surface behind the first, by admitting light behind, and by gradually removing the surfaces from each other, or bringing them into contact.

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* *Read before the Royal Scottish Society of Arts 11th April 1842.*

A convenient method of executing this is to have two stretching frames made perfectly level, to join them together at the bottom by a hinge of thin leather, to cover the faces in contact with paper, or linen, and to adapt the groove of the box to receive them (as well as the ordinary single frames), and to admit of the second frame being moved backward and forward at the top. This motion may be communicated by a thin lever passing through the bottom of the box, close to the side next to the exhibitor, immediately behind the second frame. The effect thus produced is, however, not a correct representation of fog.

A more simple and easy method of producing a representation of fog, which is correct, is to use an ordinary single frame only, to paint the objects intended not to be affected by the fog on the front of the frame (painting behind them also if necessary to produce sufficient opacity), and the rest of the scene on the back of it, to admit light in front, and gradually either to admit light behind, by which the parts of the scene painted on the back will appear to emerge from the fog, or to exclude that light, by which they will appear to become involved in the fog.

G. TAIT.

EDINBURGH, 11*th April* 1842.

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## *On British Fossil Reptiles.* By PROFESSOR OWEN.

A RETROSPECTIVE glance at the catalogue of reptiles which formerly existed on that portion of the earth's surface constituting the present small island of Britain, and which are now extinct, must call forth such novel and surprising reflections on the dealings of Providence with the animated beings of this planet, as may well lead, in the first place, to a questioning of the truth of the affirmations with which the present summary commences. Did the numerous, strange, and gigantic representatives of the several orders of reptiles actually at any time live and move, and propagate their kind in the localities where their bones are now so abundantly found? Are not these bones the relics rather of antediluvian creatures, which perished in the great historical catastrophe of water, and have been washed from latitudes suitable to their existence to more northern shores? Are the British fossil Reptiles actually extinct, and may not some living representatives of the Labyrinthodons, Enaliosaurs, Dinosaurs, &c., still remain to be discovered, in those warmer regions where alone large species of reptiles are now known to exist?

Such questions and explanations of the phenomena which are the subject of the present report, will be most likely to suggest themselves to those who are not conversant with the truths of geology, and who may never have been eye witnesses of the circumstances under which fossil bones of reptiles are found.

In many cases, these circumstances are so opposed to any that can be conceived to have been produced by the operation of a superincumbent bed of waters upon the present surface of the earth, during a period of less than one year, that the earliest observers, to whom the operations of a temporary general deluge suggested the first explanation of the appearance of the remains of a large and strange animal, were irresistibly led to the conviction, that the conditions under which such fossil animal was found, were wholly inexplicable on the supposition of its carcase having been left by the retiring waters of a flood. Thus the good Quaker of Whitby, in his letter to Dr Fothergill, recounting the discovery of the extinct species of crocodile that now bears his name (*Teleosaurus Chapmanni*), says: "The bones were covered five or six feet with water every full sea, and were about nine or ten yards from the cliff, which is nearly perpendicular, and about sixty yards high, and is continually wearing away by the sea washing against it; and, if I may judge by what has happened in my own memory, it must have extended beyond these bones less than a century ago. There are several regular strata, or layers of stone, of some yards thickness, that run along the cliff nearly parallel to the horizon and to one another. I mention this to obviate an objection, that this animal may have been upon the surface, and in a series of years may have sunk down to where it lay, which will now appear impossible, at least when the stones, &c. have had their present consistence."[*]

It must be obvious, indeed, that the regular succession of horizontal layers—"beginning from the top, of earth, clay, marble, stones, both hard and soft, of various thicknesses, till it comes down to the black slate or alum rock,"[†]—mounting to the height of near 200 feet above the petrified skeleton, could not have been the result of the deposit of a temporary overflow by diluvial waters continuing for a few months, supposing even those waters to have been thickly charged with the ruined surface of the old earth. Succession of strata, as of all other phenomena, must take place in successive periods of time; the hundredth layer of lias, counting downwards, which contained the skeleton of the strange crocodile, must once have been the uppermost; and some time must have elapsed between the deposition of that stratum with its organized contents, and the deposition of the succeeding layer above.

If the fossilized bones of the animals described in the present memoir had been drifted to this island by a flood, they would be found mingled together in the superficial strata usually termed "diluvial," and would characterize no particular formation or locality. But the reverse of this is the fact; and it is the cumulative evidence of the limitation of certain genera to particular formations, that gives its chief value to the present class of researches.

In the most superficial fossiliferous deposits, which indicate the last operation of a body of water, either frozen or fluid, upon the surface of the British islands, no remains of reptiles have come under my observation. Cuvier alludes to a single bone of a crocodile said to have been found associated with the usual fossils of the drift or diluvium at Brentford;[‡] but no other evidence of any other species or genus of reptile, which is now confined to warmer regions of the globe, has yet been recognised in the British strata called diluvial, or in any that are more recent than the oldest tertiary formations.

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[*] Philosophical Transactions, 1758, p. 688. [†] Ibid, p. 789.

[‡] Dr Buckland has suggested to me that this bone was probably washed out of the clay beneath the diluvium.

In these eocene beds, accumulated in some localities to the thickness of 300 feet and upwards, the remains of crocodiles, tortoises, trionyxes, turtles, and large serpents, are more or less common. These fossils severally exhibit well-marked and unequivocal specific differences when compared with the bones of their known existing congeners ; but their osteology does not present any modifications of generic value. The nearest approach to this degree of deviation occurs in the eocene chelonian reptiles, as in that species of turtle from Sheppey, which combines the jaws of a trionyx with the bony helmet of a turtle, and presents an extent of ossification of the buckler nearly equalling that of an emys. The eocene crocodile exhibits all the characters of the osseous and dental systems which distinguish the genus as restricted in the latest systems of erpetology ; and whilst it cannot be identified with any known species, most resembles, not the commonest and nearest existing crocodile, as that of the Nile, but a rarer and more remote one, viz., the *Crocodilus Schlegelii* of Borneo. Not any species of reptile of the tertiary strata has been discovered in the chalk upon which those strata immediately rest.

A small lizard, closely corresponding in vertebral structure with existing species, but differing in its dentition ; and a gigantic marine species (*Mosasaurus*), which is the first, in the present descending survey, to offer osteological and dental combinations wholly unknown in existing saurians,—constitute the representatives of the lacertian order in the cretaceous beds, which form the most recent of the secondary deposits.

In tracing upwards the extinct reptiles, we find that the union of the vertebræ by a hinder ball received into an anterior cup, a structure which, with an insignificant exception—the gecko—prevails throughout the saurian order as it now exists, commences with the lacertian reptiles which perished during the deposition of the chalk, and, in the crocodilian and ophidian reptiles, is first found in the species which made their appearance during the deposition of the London clay.

Of the crocodilian order I have yet seen no unequivocal representatives from the British chalk.

All the well-determined Chelonians of the cretaceous period are marine species, and are equally distinct, with the lacertians, from those of the super-imposed tertiary beds.

The most interesting fact which the palæontology of the cretaceous period has yielded, with reference to the great saurian division of the class of reptiles, is the commencement, or rather the last appearance of the fossil remains of an order of reptiles (*Enaliosauria*) now altogether extinct. I have exaxamined portions of the lower jaw with teeth of a large species of *Ichthyosaurus* from the lower chalk between Folkstone and Dover, which is very closely allied to, if not identical with, the *Ichthyosaurus communis.* The femur of a large Plesiosaurus has been obtained from the chalk of Shakspeare's Cliff. Remains of more than one species of plesiosaurus occur in the gault, and are associated with the *ichthyosaurus* in the greensand near Cambridge, and in the Kentish Rag near Maidstone.

In the green-sand also we first meet with evidences of reptiles exhibiting modifications of structure, especially of the locomotive extremities, as remarkable and as different from those of existing species as are presented by the *Enaliosauria,* but pointing as strongly to an adaptation for terrestrial life as does the enaliosaurian structure to aquatic existence. The specimen of the unquestionably terrestrial saurian here alluded to, viz., the *Iguanodon,* is the more remarkable in the subcretaceous marine strata, in consequence of its presenting the largest portion of the connected skeleton of the same individual of this species that has hitherto been found.

Gigantic crocodilian reptiles, removed by generic modifications of structure from the eocene and existing crocodiles, now likewise begin to be indicated, as by the teeth of the *Polyptychodon* from the green-sand quarry at Maidstone, and by the large bones of the extremities from the quarries of a corresponding stratum at Hythe.

The chelonian from the greensand (*Chelone pulchriceps*) differs from the eocene and all existing turtles in a very interesting modification of the anatomy of the cranium.

In the Wealden group of fresh-water strata, the Enaliosaurian order continues to be represented by the *Plesiosaurus,* but no remains of the more strictly marine genus, Ichthyosaurus, have yet been discovered. This circumstance corresponds with the more strict adaptation for marine existence which the structure of the *Ichthyosaurus* presents, and corroborates the inference that the *Plesiosaurus* lived nearer the shore, and ascended estuaries. The reappearance of the *Ichthyosaurus* in the chalk formations proves that it had continued to exist in the neighbouring ocean, and indicates, perhaps, that the deposition of the cretaceous beds was related to the formation of the Wealden group by proximity of time as well as place. The terrestrial group of gigantic reptiles receives in the Wealden an accession of two new genera, viz., *Hylæosaurus* and *Megalosaurus;* and the remains of both these, and especially of the *Iguanodon*, are so abundant, that the Wealden strata may be regarded as the metropolis of the Dinosaurian order.*

The amphibious crocodiles might be expected, from their known habits at the present day, to have left abundant evidences of their remains in strata which seem to have been deposited at the estuary or mouth of some great river; in a climate, indicated by its vegetable fossils to have been warmer or more equable than at present; and during a period of time which permitted the accumulation of 1000 feet of strata. Accordingly, the crocodilian order of reptiles has been found to be represented by several distinct genera in the Wealden formations.

Some new characters and modifications of structure might also have been anticipated in those crocodilians which existed at a period antecedent to the deposition of about 1500 feet of cretaceous strata, which again preceded the formation of the whole series of superimposed tertiary and diluvial beds. Nevertheless, the remarkable modifications which all the Wealden crocodilians present in the structure of their vertebræ, as compared with the eocene and existing crocodiles, could not have been anticipated; and even now that they are ascertained by repeated observation, some of them still remain inexplicable in relation to any conjectural habits of the species, or hypothetical conditions under which they actually existed. We may understand why the ball and socket articulation of the vertebræ of the present amphibious crocodiles frequenting dry land, should be exchanged for a joint having elastic substances included between two concave articular surfaces, as a structure better adopted to crocodiles more habitually living and moving in water; but these crocodiles with biconcave vertebræ are associated with others having plano-concave vertebræ, and also with a species having vertebræ joined by ball and socket articulations. And the difficulty is not diminished by the remarkable fact of the latter structure being the reverse of that in recent crocodiles, the ball and the cup having changed places in the extinct *Streptospondylus;* and having assumed the position which they present in certain sauroid fishes, and in the dorsal and cervical vertebræ of some of the herbivorous mammalia.

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* Dr Mantell calculates that not less than seventy individuals of the *Iguanodon*, varying in age and magnitude, from the young just escaped from the shell to the mature animal, with a femur of more than a yard in length, have come under his examination; and he justly observes that "more than thrice that number have, in all probability, been destroyed by the workmen, and altogether eluded the observation of the palæontologist." See his Memoir in the Philosophical Transactions, 1841.

The biconcave, plano-concave, and convexo-concave modifications of the vertebræ, are not the only points in which the extinct crocodilians of the Wealden strata differ from those of the London clay and from the existing species. The genus *Goniopholis*, for example, exhibits a remarkable development of its dermal armour, the large quadrangular scutes of which, interlocked by teeth received into depressions, are gigantic representations of the scales of some of the Ganoid fishes; while the smaller hexagonal and pentagonal scutes* were articulated together by marginal sutures, as in the dermal bony covering of the armadillo. The *Poikilopleuron* exhibits a medullary cavity in the body of the vertebræ, and a double origin of the spinous process. The *Suchosaurus* offers a very striking change in the form of the teeth. The *Cetiosaurus* surpasses all modern crocodiles in its enormous bulk, which almost rivals that of the whales, its successors in the modern seas. The genus *Streptospondylus*, which, in repeating the ball and socket structure, offers the strange anomaly of an anterior position of the ball and a posterior one of the socket, makes its first appearance in the Wealden by a species which must have been little inferior to the *Cetiosaurus* in length.

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* These have been discovered since the first sheets of this Report went to press, by my friend Mr Holmes of Horsham, in the Wealden strata near that town.

The huge terrestrial saurians of the Wealden deviate in so much greater a degree than the crocodilians from the existing types, as to render the formation of a distinct order for their reception necessary.

It does not appear that any of the Chelonians of the Wealden period are specifically identical with those of the chalk. A new and singular osculant genus, *Tretosternon*, here represents the *Trionyces* of the eocene fresh water or estuary formations. A new species of turtle with an emydian form of shell, occurs in the Purbeck limestone; and the head of a turtle from the Portland stone, upon which the Purbeck beds immediately rest, exhibits the same distinction of the separate nasal bones as does the skull of the turtle from the greensand, but combined with well-marked specific differences in other parts.

The Portland stone introduces us to the great oolitic series, in which we lose sight of the *Iguanodon*, *Hylæosaurus*, *Goniopholis*, and *Suchosaurus*, but find that the *Megalosaurus*, *Poikilopleuron*, *Cetiosaurus*, *Streptospondylus*, and *Plesiosaurus*, are genera common to the Wealden and oolitic periods.

Now also the genus *Ichthyosaurus*, which was represented by a single species in the chalk epoch, astonishes us by the number of individuals, and the great variety of specific modifications and varieties of form and bulk, under which it existed in the oolitic periods, especially in the older divisions of the oolite, as the lias. The number and variety of plesiosaurian reptiles are even more surprising, and the modifications of their skeleton being more marked and various, proportionally facilitate the determination of the species. The largest of these plesiosaurian reptiles deviates, indeed, so far from the typical structure of the genus, as to merit sub-generic distinction. This sub-genus, the *Pliosaurus*, characterises the Kimmeridge and Oxford clays, but appears not to have existed at the period of the lower oolite.

In the place of the *Goniopholis* and *Suchosaurus*, the crocodilian genera, *Steneosaurus* and *Teleosaurus*, with the sub-genera *Ælodon*, *Mystriosaurus*, *Macrospondylus*, &c. (separated, perhaps, without sufficient reason, from the first two typical genera of amphicœlian crocodiles), make their appearance in the oolitic strata, especially in the lower divisions. The long and narrow snouts, sharp and slender teeth, short fore-limbs, and imbricated scutation of these extinct crocodilians, attest, with their vertebral structure, their adaptation to an aquatic life, and to the capture of a prey not more highly organized than fishes.

Some small species of crocodilians and lacertians have left a few bones of their extremities in the oolitic slate of Stonesfield; and a most singular order of reptiles now makes its appearance, the skeleton of which exhibits a modification of the lacertian type of structure closely analogous to that by virtue of which the mammalian bat is endowed with the powers of flight. The flying dragons, called *Pterodactyli*, were of small size, and are restricted, like the *Teleosauri* and *Steneosauri*, to the oolitic group. All the other genera are continued into the Wealden,—the *Poikilopleuron* and *Megalosaurus*, by identical species,—the other genera by species which are distinct from those of the oolite. The *Plesiosaurus* and *Ichthyosaurus*, existed, as we have seen, as late as the deposition of the chalk. The analogy between the extinct reptiles and fishes, in regard to the great proportion of genera which are common to the Wealden and oolite, and the small proportion which is continued into the cretaceous formations, offers a valuable corroboration of the subordinate character of the Wealden group as a member of the great oolitic series.

No species or genus of saurian, represented by fossils from the oolite, has yet been discovered in older or lower strata in the British Islands. The *Rysosteus* is apparently confined to the bone bed under the lias, which may be regarded as the oldest member of the oolitic series in these islands.

The reptiles of the *Poikilitic* strata exhibit deviations from the typical structure of the recent families, together with osculant characters joining groups now distinct, as great and even more anomalous than occur in any of the preceding extinct *genera*.

The *Rhynchosaurus* of the new red-sandstone near Shrewsbury manifests ornithic and chelonian modifications, grafted upon an essentially lacertian type of cranial structure; no approach even to the form of its extraordinary head being made by any other of the extinct members of the saurian order. The vertebræ of the *Rhynchosaurus* differ from those of existing lizards, chelonians, and birds, and combine the biconcave structure with the oblique processes and costal articulations of the vertebræ of recent lizards.

The Labyrinthodonts of the same formation exhibit a different but an equally remarkable combination of characters, crocodilian modifications being superinduced upon a fundamental organization of the Batrachian type. The structure of the teeth in this remarkable family, which is the most complex that has hitherto been met with in the whole animal kingdom, is unique in the class of reptiles, and is but partially and comparatively feebly repeated in that of fishes. It is highly probable that the modifications of the locomotive extremities were as peculiar as the dental character, if we may judge from the foot-prints of the so-called *Cheirotherium*, to which the Labyrinthodonts alone have at present an equitable claim.

Finally, the Palæosaurus, and other genera of the magnesian conglomerate, like the so-called monitors of Thuringia, are lizards, which combined a thecodont type of dentition with biconcave vertebræ, having the superadded peculiarity of a series of ventricose excavations in the bodies of the vertebræ for the spinal chord, instead of a cylindrical canal.

Below the new red sandstone system, notwithstanding that the older deposits, as the coal measures, have been more thoroughly explored by man than any other geological formation, no trace of a vertebrate animal more highly organized than a fish has been detected.

From this survey it is evident, that many races of extinct reptiles have succeeded each other as inhabitants of the portion of the earth now forming Great Britain; their abundant remains, through strata of immense thickness, shew that they existed in great numbers, and probably for many successive generations. Their coprolites prove, that they fed upon organized beings coexisting with them, and characterizing the same strata, but now equally extinct with their devourers.

To what natural or secondary cause, it may then be asked, can the successive genera and species of reptiles be attributed? Does the hypothesis of the transmutation of species, by a march of development occasioning a progressive ascent in the organic scale, afford any explanation of these surprising phenomena? Do the speculations of Maillet, Lamarck, and Geoffroy, derive any support, or meet with any additional disproof, from the facts already determined in the reptilian department of palæontology?

A slight survey of organic remains may, indeed, appear to support their views of the origin of animated species; but of no stream of science is it more necessary, than of palæontology, "to drink deep or taste not."*

Of all vertebrated animals, the reptiles form the class which is least fixed in its characters, and is most transitional in its range of modifications; the lowest organized species are hardly distinguishable from fishes, and the highest manifest so great an advance in all the important systems of their organism, that naturalists are not yet agreed as to whether reptiles ought to remain in one class or form two. Reptiles are, besides, the only class of vertebrate animals in which certain species undergo, after birth, a metamorphosis as singular and extreme as in insects.

If the progressive development of animal organization ever extended beyond the acquisition of the mature characters of the individual, so as to abrogate fixity of species by a transmutation of a lower into a higher organization, some evidence of it ought surely to be obtained from an extensive and deep survey of that class of animals which, whilst intermediate in organization between fishes and mammals, prevailed most on the earth during the long periods that intervened between the time when the only vertebrate animals were fishes, and the tertiary and modern epochs, when mammals have become abundant, and have almost superseded reptiles in the herbivorous and carnivorous departments of the economy of nature.

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* The following are the latest terms in which the transmutation theory has been promulgated, as supported by palæontology:—"The life of animals exhibits a continued series of changes, which occupy so short a period, that we can generally trace their entire order of succession, and perceive the whole change of their metamorphoses. But the metamorphoses of species proceed so slowly with regard to us, that we can neither perceive their origin, their maturity, nor their decay; and we ascribe to them a kind of perpetuity on the earth. A slight inspection of the organic relics deposited in the crust of the globe, shews that the forms of species, and the whole zoology of our planet, have been constantly changing; and that the organic kingdoms, like the surface they inhabit, have been gradually developed from a simpler state to their present condition." Dr Grant's Lectures on Comparative Anatomy.—*Lancet*, 1835, p. 1001.

In accordance with this not unreasonable expectation, the reptiles of the magnesian conglomerate and new red sandstone ought to have been organized according to the type of the most fish-like perennibranchiate Batrachians; and the fishes of the older strata, if they tended to a higher stage of development, ought, upon achieving the passage to the reptilian class, to have entered it at its lowest step.

It is true, indeed, that the most characteristic reptilian remains of the new red sandstone do belong essentially, as by their double occipital condyle, their vomerine palate bones and teeth, &c., to the Batrachian order; but had the Labyrinthodonts now existed, instead of ranking as the lowest members of that order, they would most unquestionably have been esteemed the highest; and, as in the existing diversified order of Batrachia, one family[*] represents fishes, a second[†] serpents, a third genus[‡] Chelonians, and a fourth[§] lizards; so would the now lost Labyrinthodonts have formed batrachian representatives of the highest order of reptiles, viz. the crocodilians. Here, therefore, we find the batrachian making its first appearance under its highest, instead of its lowest or simplest conditions of structure. To use the figurative language of the transmutation theory, the Labyrinthodonts are degraded crocodiles, not elevated fishes.

But the hypothetical derivation of reptiles from metamorphosed fishes is more directly negatived by the fact, that the batrachian type is not that under which reptiles make their first appearance in the strata which succeed the coal measures.—The monitors of the Thuringian Zechstein are older than the labyrinthodonts of the Keuper; and among British reptiles, the thecodont lizards of the magnesian conglomerate have equal claims to a more ancient origin.

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[*] *Perennibranchiata.*
[†] Ceciliadæ.
[‡] Pipa.
[§] Salamandra.

The questions, which the unbiased collector of evidence bearing upon the fixity or mutability of species has next to resolve respecting these primeval lizards, are, whether they appeared under the form of the low organized species, which one naturalist classes with *Sauria*, another with *Ophidia*, or whether they exhibit indications of having emerged, by progressive development of structure, from any lower organized pre-existing group of cold-blooded animals? To these inquiries the palæontologist must reply, that the thecodont lizards of the zechstein and magnesian conglomerates combine well organized extremities, with teeth implanted in distinct sockets, instead of being soldered, as in frogs, to a simple alveolar parapet; and that, therefore, if they existed at the present day, they would take rank at the head of the Lacertian order, and not among the families most nearly allied to the inferior reptiles. Neither are the modifications of the skeleton of the Rhynchosaur, from the new red sandstone, such as indicated that singular lacertian to have been derived from the Ophidian or Batrachian orders; but, on the contrary, they connect it more closely than any known recent species with Chelonia and birds.

The nearest approximation to the organization of fishes is made by the *ichthyosaurus*, an extinct genus, which appears to have been introduced into the ancient seas, subsequent to the deposition of the strata inclosing the remains of the thecodont lizards. The ichthyic characters of this genus of marine saurians are not of a very important kind, being limited, like the modifications of the mammalian type in whales, to a relationship with locomotion in water, while all the modifications of the skeleton which are connected with the respiratory, digestive, or generative functions, are conformable with the highest or saurian type of reptiles; such as the cranial anatomy, the large size of the intermaxillary bones excepted; the dental structure, which corresponds with that of the posterior teeth in alligators; the articulation of the neurapophyses to the bodies of the vertebræ; the complicated pectoral arch; the sternum, and complete abdominal cincture of ribs,* &c. The circle of numerous imbricated sclerotic bones reaches its maximum of development in the *ichthyosaurus;* but this is an exaggeration of a structure feebly shadowed forth in some existing saurians, and more strongly shewn in birds, rather than a repetition of the simple bony sclerotic cup in fishes. By no known forms of fossil animals can we diminish the wide interval which divides the most sauroid of fishes from an *ichthyosaurus.*

This most extraordinary reptile is a singular compound, in which ichthyic, cetacean, and ornithic characters are engrafted upon an essentially saurian type of structure. The *ichthyosaurus* is, therefore, just such a form of animal as might be expected, were specific forms unstable, to demonstrate a mutation of characters, or some tendency towards a progressive development into a higher and more consistent type of organization. Nor is the field for testing the transmutation theory less ample than the subject is favourable. We have the opportunity of tracing the *ichthyosauri*, generation after generation, through the whole of the immense series of strata which intervene between the new red sandstone and the tertiary deposits. Not only, however, is the generic type strictly adhered to, but the very species, which made its first abrupt appearance in the lowest of the oolitic series, maintains its characters unchanged and recognizable in the highest of the secondary strata. In the chalk formations, for example, the genus *ichthyosaurus* quits the stage of existence as suddenly as it entered in the lias, and with every appreciable osteological character unchanged.

Of the different species of the *ichthyosaurus,* founded upon minor modifications of the skeleton, several appear contemporaneously in the strata where the genus is first introduced; and those which remain the longest manifest as little change of specific as generic characters. There is no evidence whatever that one species has succeeded, or been the result of the transmutation of a former species. The tenuirostral *ichthyosaurus* existed at the same time, and under the same external influences, as the stronger and shorter jawed *Ichthyosaurus communis*; just as the tenuirostral *Delphinus Gangeticus* co-exists at present with the short-jawed porpoise.

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* This structure proves that the mode of generation of the *ichthyosaurus* must have resembled that of the crocodile, and not that of the batrachians or fishes.

If the relative periods of existence of the different *Enaliosaurian* reptiles were not well ascertained, and room were allowed for conjecture as to their successive appearance on this planet, it would be as easy as seductive to speculate on the metamorphoses by which their organic frame-work, influenced by varying conditions, during a lapse of ages, might have been gradually modified, so as to have successively developed itself from an ichthyosaur to a plesiosaur, and thence to a crocodile.

We may readily conceive, for example, the fish-like characters of the vertebral column of the *ichthyosaurus* to have been obliterated by a filling up of the intervertebral cavities through ossification of the intermediate elastic tissue, and the plesiosaurian type of vertebra to be thus acquired. The normal digits of the fin might be supposed to become strengthened and elongated by more frequent reptation on dry land, and thus to cause an atrophy of the supernumerary fingers; phalanges of a more saurian figure might have been produced by the confluence of a certain number of digital ossicles; the head might be shortened by a stunted growth of the intermaxillary bones, and thus be reduced to plesiosaurian proportions. The teeth might become more firmly fixed by the shooting of bony walls across their interspaces, as in the young crocodiles. If we now elongate the bodies of the vertebræ, reduce some twenty pairs of anterior ribs to hatchet bones, place the fore-paddles at a corresponding distance from the head, and the hind-paddles proportionally nearer the end of the tail, little more will be required to complete the transmutation of the ichthyosaur into the plesiosaur.

If next, in adaptation to a gradual change of surrounding circumstances, the jaws of the Plesiosaur became lengthened to the proportions of those of the tenuirostral Ichthyosaur, but at the expense of maxillary, instead of the intermaxillary bones, preserving the socketed implantation of the teeth; if, to balance the elongation of the jaws, the neck at the same time shrunk to nearly its former Ichthyosaurian proportions, with some slight modifications of the Plesiosaurian type of the vertebræ; if a further development and a more complete separation of the digits of the fore and hind members were to take place, so that they might serve for creeping as well as swimming; if the exposure of the surface to two different media, and of the entire animal to perils of land as well as of sea, were to be followed by the ossification of certain parts of the skin, and the acquisition, by this change, of a dermal armour, such we might conceive to be the leading steps in the transmutation of the Plesiosaur into the Teleosaur.

And if the three forms of extinct Saurians, whose changes of specific and generic have thus been speculated on, had actually succeeded each other in strata successively superimposed in the order in which they have here been hypothetically derived from one another, some colour of probability might attach itself to this hypothesis, and there would be ground for searching more closely into the anatomical and physiological possibilities of such transmutations. *Ichthyosaurus*, *Plesiosaurus*, and *Teleosaurus* are, however, genera which appeared contemporaneously on the stage of vital existence; one neither preceded nor came after the other. How the transmutation theory is to be reconciled to these facts is not obvious; nor to these others, viz. that the Teleosaur ceases with the oolite, while the Ichthyosaur and Plesiosaur continue to coexist to the deposition of the chalk, and disappear together alike unchanged; the Ichthyosaur manifesting as little tendency to develope itself into a Plesiosaur, as this to degrade itself into the more fish-like modification of the Enaliosaurian type.

If it were urged that the *Streptospondylus*, or crocodile with ball and socket vertebræ, of which the remains occur in later secondary strata, when the Teleosaur had ceased to exist, might be a modification of the apparently extinct Amphicælian crocodile, in which the vertebræ had undergone a progressive development, analogous to that by which the biconcave joints of the vertebræ of the tadpole are actually converted into the ball and socket joints of those of the mature frog, the facts of both geology and anatomy again oppose themselves to such an hypothesis; for the remains of the Streptospondylus occur likewise in the Whitby lias, which is the earliest formation characterized by remains of the Teleosaurus; and the modifications of the vertebral structure, by which the *Streptospondylus* differs from its ancient contemporary, and which it retains unaltered throughout the whole series of oolitic strata, is no approximation to the ball and socket structure of modern crocodiles, which first appears in the *Mosasaurus* and the Eocene crocodiles, but is the very reverse. As reasonably might we infer that the Teleosaur was an intermediate form between the Streptospondylus and modern crocodiles, and that the anterior ball had first subsided, and a sub-biconcave type of vertebræ had been produced before the posterior ball, which characterizes the vertebræ of recent crocodiles, was finally developed.

If the present species of animals had resulted from progressive development and transmutation of former species, each class ought now to present its typical characters under their highest recognized conditions of organization; but the review of the characters of fossil reptiles, taken in the present Report, proves that this is not the case.

No reptile now exists which combines a complicated and thecodont dentition with limbs so proportionally large and strong, having such well-developed marrow bones, and sustaining the weight of the trunk by synchondrosis or anchylosis to so long and complicated a sacrum, as in the order *Dinosauria.*

The Megalosaurs and Iguanodons, rejoicing in these undeniably most perfect modifications of the reptilian type, attained the greatest bulk, and must have played the most conspicuous parts, in their respective characters as devourers of animals and feeders upon vegetables, that this earth has ever witnessed in oviparous and cold-blooded creatures. They were as superior in organization and in bulk to the crocodiles that preceded them as to those which came after them.

There is not the slightest ground for affirming that the procœlian Gavial of the present day is in any respect more highly organized than the opisthocœlian Gavial of the oldest lias. If the differences of vertebral structure in these crocodilians were contrasted, in reference to their relative approximation to the vertebral structure of the higher animals, the resemblance of the ball and socket joints of the spine of the Streptospondylus to those of certain mammals would give precedence in organic perfection to the primeval Gavial.

If, therefore, the extinct species, in which the reptilian organization culminated, were on the march of development to a higher type, the *Megalosaurus* ought to have given origin to the carnivorous mammalia, and the herbivorous should have been derived from the *Iguanodon*. But where is the trace of such mammalia in the strata immediately succeeding those in which we lose sight of the relics of the great Dinosaurian reptiles? Or where, indeed, can any mammiferous animal be pointed out whose organization can, by any ingenuity or licence of conjecture, be derived without violation of all known anatomical and physiological principles, from transmutation or progressive development of the highest reptiles.

If something more than a slight inspection be bestowed upon the organic relics deposited in the crust of the globe, we learn that the introduction of the mammalia on that crust is independent of the appearance of the highest forms of reptiles. The small insectivorous mammals of the lower oolite* are contemporary with the most ancient Dinosaur, and are anterior to the *Iguanodon*.

The period when the class of reptiles flourished under the widest modifications, in the greatest number and of the highest grade of organization, is past; and, since the extinction of the Dinosaurian order, it has been declining. The reptilia are now in great part superseded by higher classes: Pterodactyles have given way to birds; Megalosaurs and Iguanodons to carnivorous and herbivorous mammalia; but the sudden extinction of the one, and the abrupt appearance of the other, are alike inexplicable on any known natural causes or analogies.

New species, genera, and families of reptiles have constantly succeeded each other since the earliest periods in which the remains of this class can be discerned; but the change has been, upon the whole, from the complicated to the simple. The Batrachian order, which is first indicated by the large and powerful crocodiloid *Labyrinthodonts*, has dwindled down to the diminutive and defenceless Anourans and the fish-like Perennibranchians. The Saurian order was anciently represented by reptiles manifesting the crocodilian grade of organization, under a rich variety of modifications and with great development of bulk and power; it has now subsided into a swarm of small Lacertians, headed by so few examples of the higher or loricate species, that it is no marvel such relics of a once predominating group should have found a humble place in Linnæus's catalogue of nature as co-ordinate members of the genus *Lacerta*.

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* For the proof of the often doubted mammalian character of the Thylacotherium and Phascolotherium of the Stonesfield slate, the reader is referred to the memoirs in the sixth volume of the second series of the Geological *Transactions*, pp. 47–58.

Nevertheless, some general analogies may be traced between the phenomena of the succession of reptiles as a class, and those observed in the development of an individual reptile from the ovum. Thus the embryonic structure of the vertebræ of the existing crocodiles accords with the biconcave type; and this is exchanged, in the development of the individual as in the succession of species, for the ball and socket structure as the latest condition.

The almost universal prevalence of the more or less biconcave structure of the vertebræ of the earlier reptiles, thus establishes a most interesting analogy between them and the earlier stages of growth of existing reptiles.

A similar analogy has been pointed out by M. Agassiz, between the heterocercal fishes, which exclusively prevail in the oldest fossiliferous strata, and the embryos of existing homocercal fishes, which seem to pass through the heterocercal stage. The superior number of loricate reptiles, and the more complete development of the dermal armour in the crocodilian genera *Steneosaurus*, *Teleosaurus*, *Goniopholis*, &c., of the oolitic and Wealden strata, corresponds with the prevalence of the well-mailed Ganoid order of fishes in the same formations.

The fossil reptiles, like the fossil fishes, approximate nearest to existing species in the tertiary deposits, and differ from them *most widely in strata* whose antiquity is highest.

Not a single species of fossil reptile now lives on the present surface of the globe.

The characters of modern genera cannot be applied to any species of fossil reptile in strata lower than the tertiary formations.

No reptile, with vertebræ articulated like those of existing species, has been discovered below the chalk.

Some doubt may be entertained as to whether the *Ichthyosaurus communis* did not leave its remains in both oolitic and cretaceous formations; but, with this exception, no single species of fossil reptile has yet been found that is common to any two great geological formations.

The evidence acquired by the researches which are detailed in the body of this Report, permits of no other conclusion than that the different species of reptiles were suddenly introduced upon the earth's surface, although it demonstrates a certain systematic regularity in the order of their appearance. Upon the whole, they make a progressive approach to the organization of the existing species, yet not by an uninterrupted succession of approximating steps. Neither is the progression one of ascent, for the reptiles have not begun by the perennibranchiate type of organization, by which, at the present day, they most closely approach fishes; nor have they terminated at the opposite extreme, viz. at the Dinosaurian order, where we know that the reptilian type of structure made the nearest approach to mammals.

Thus, though a general progression may be discerned, the interruptions and faults, to use a geological phrase, negative the notion that the progression has been the result of self-developing energies adequate to a transmutation of specific characters; but, on the contrary, support the conclusion that the modifications of osteological structure which characterize the extinct reptiles, were originally impressed upon them at their creation, and have been neither derived from improvement of a lower, nor lost by progressive development into a higher type.

The general progressive approximation of the animal kingdom to its present condition has been, doubtless, accompanied by a corresponding progress of the inorganic world; and thus, the differences which comparative anatomy demonstrates to have existed between the vertebrated inhabitants of the secondary epochs of the geological history of the earth, and the tertiary and present periods, form legitimate grounds for speculation, not only on the essential nature and causes of those differences, but upon the progressive changes to which our planet and its atmosphere may have been subject. For, whether there had been grounds for regarding the organic phenomena of primeval times as earlier stages in the progressive development and transmutation of species, or whether, as the closest investigation of these phenomena seems to demonstrate, they have been the result of expressly created and successively introduced species,—they naturally lead the physiologist to speculate on the varying conditions of the surrounding media to which such organic differences may have related.

Now, reptiles mainly and essentially differ from birds and mammals in the less active performance of the respiratory function, and in a lower and simpler structure of the lungs and heart, whereby they become, so to say, less dependent on the atmosphere or oxygen for existence. From their extraordinary prevalence in the secondary periods, under varied modifications of size and structure, severally adapting them to the performance of those tasks in the economy of organic nature which are now assigned to the warm-blooded and quick-breathing classes, the physiologist is led to conjecture that the atmosphere had not undergone those changes, which the consolidation and concentration of certain of its elements in subsequent additions to the earth's crust may have occasioned, during the long lapse of ages during which the extinction of so large a proportion of the reptilian class took place. And if the chemist, by wide and extended views of his science in relation to geology and mineralogy, should demonstrate, as the botanist, from considerations of the peculiar features of the extinct Flora, has been led to suspect, that the atmosphere of this globe formerly contained more carbon and less oxygen than at present, then the anatomist might, *à priori*, have concluded that the highest classes of animals suited to the respiration of such a medium must have been the cold-blooded fishes and reptiles.

And, besides the probability of such a condition of the zoological series being connected with the chemical modifications of the air, the terrestrial reptiles, from the inferior energy of their muscular contractions, and still more from the greater irritability of the fibres, and power of continuing their actions, would constitute the highest organized species, best adapted to exist under greater atmospheric pressure than operates on the surface of the earth at the present time.

Through such a medium, approaching in a corresponding degree to the physical properties of water, a cold-blooded animal might even rise above the surface and wing its heavy flight, since this would demand less energetic muscular actions than are now requisite for such a kind of locomotion ; and thus we may conceive why the atmosphere of our planet, during the earlier oolitic periods, may have been traversed by creatures of no higher organization than Saurians. If we may presume to conjecture that atmospheric pressure has been diminished by a change in the composition as well as by a diminution of the general mass of the air, the beautiful adaptation of the structure of birds to a medium thus rendered both lighter and more invigorating, by the abstraction of carbon and an increase of oxygen, must be appreciable by every physiologist. And it is not without interest to observe, that the period when such a change would be thus indicated by the first appearance of birds in the Wealden strata,* is likewise characterized by the prevalence of those dinosaurian reptiles which in structure most nearly approach mammalia, and which, in all probability, from their correspondence with crocodiles in the anatomy of the thorax, enjoyed a circulation as complete as that of the crocodile when breathing freely on dry land.*

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* Foot-prints alone, like those termed "Ornithichnites," observed in the new red sandstone of Connecticut, are insufficient to support the inference of the possession of the highly developed organization of a bird of flight by the creatures which have left them. The Rhynchosaur and biped Pterodactyles already warn us how closely the Ornithic type may be approached without the essential characters of the Saurian being lost. By the Chirotherian Ichnolites we learn how closely an animal, in all probability a Batrachian, may resemble a pedimanous mammal in the form of its foot-prints.

The degree in which flying insects can resist noxious gases, which would be quickly fatal to the warm-blooded vertebrates, invalidates the objection to a progressive change of atmosphere having accompanied the prevalence of quick breathing animals, which might be suggested by the *Libellulæ* of the lias and by the oolitic beetles.

The first indications of the warm-blooded classes, it might be anticipated, would appear, if introduced into the reptilian era, under the form of such small insectivorous mammals, as are known at the present day to have a lower amount of respiration than the rest of the class; and the earliest discovered remains of mammalia, as for example, those in the Stonesfield oolite, are actually the jaws of such species, with which are combined the characters of that order, Marsupialia, which is most nearly related to the oviparous vertebrata.

The present speculations are, however, offered with all due diffidence; the collection of the evidence requisite for pursuing them to a semblance even of demonstration is only just begun, and they are thrown out with no other expectation of utility than as incentives to the chemical consideration of the nature and possibilities of such atmospheric changes as may be physiologically connected with the variations of organic nature made known by the researches of the anatomist. A too cautious observer would, perhaps, have shrunk from such speculations, although legitimately suggesting themselves from the necessary relations between the organs and media of respiration; but the sincere and ardent searcher after truth, in exploring the dark regions of the past, must feel himself bound to speak of whatever a ray from the intellectual torch may reach, even though the features of the object should be but dimly revealed.—*Plymouth British Association Report*, p. 191.

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* All existing reptiles, which have the ribs at the anterior part of the thorax united by a head and tubercle to the centrum and neurapophysis of the vertebræ, have a heart with two distinct ventricles, as well as two auricles. The contiguous aortæ arising from the two ventricles intercommunicate by an aperture so placed as to be covered by the sigmoid valves when blood is transmitted equally through them. When the amphibious crocodile suffers an interruption in the pulmonary circulation by continued submersion, the aorta from the left ventricle, by the communication above mentioned, receives venous blood from the overcharged cavities of the right side of the heart; but when respiration is in full vigour on dry land, an undiluted stream of arterial blood is transmitted through the left aorta to the head and anterior extremities. The Dinosaurs, having the same thoracic structure as the crocodiles, may be concluded to have possessed a four-chambered heart; and, from their superior adaptation to terrestrial life, to have enjoyed the function of such a highly organized centre of circulation, in a degree more nearly approaching that which now characterises the warm-blooded vertebrata.

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## *On the Influence of Mountains on Temperature in the Winter in certain parts of the Northern Hemisphere.* By Mr Hopkins.

It was stated by Mr Hopkins, at the Plymouth Meeting of the British Association, that between the latitudes of 40° and 70° north, there is, in the same parallels, a great difference of temperature, particularly in the winter, amounting, in some cases, to as much as 40° or even 50° of Fahrenheit. The western coasts of the two continents are much warmer than the eastern, and the winds generally blow from the sea to the western coasts; and it has been inferred that the prevailing winds passing over sea to the western coasts, and over land to the eastern, was the cause of the difference in the temperature. This inference is not, however, in accordance with facts, as the low temperature is not proportional to the distance from the western coast. Throughout this part of the northern hemisphere, it is found that climate has certain relations to the elevation of land, not simply arising out of the elevation of that part of the earth's surface above the general level, but out of the influence which the elevation exercises on the atmosphere. (Here a diagram was exhibited, illustrating Hadley's theory of the atmospheric currents.) This theory represents the tropical atmosphere as rising and flowing over at the top towards the polar regions, and returning when cooled, flowing along on the surface of the earth. This inequality of temperature in the atmosphere would cause an upper current to flow north, and an under current to flow south. But the unequal velocities of the different parts of the earth's surface, from the equator to the pole, modify the course of these currents, and make the upper a south-west, and the lower a north-east current, as shewn by lines on a Mercator's chart. This theory, true in its leading principles, does not account for what occurs on the earth's surface, because it does not take in all the causes that are in operation; which causes materially modify the general results. The polar current, in flowing from the north-eastern part towards the south-west, meets with elevations of the land, and is, consequently, along a diagonal stripe in the direction of the general currents, obstructed in its progress, and sometimes stopped, and obliged to turn back, as an upper current, towards the pole; while beyond the obstruction, nearer to the equator, the tropical or upper current, not being met by a polar current along this line, flows towards the obstruction, from whence it returns partially cooled as an under current. The consequence is, that along such a stripe, the great atmospherical currents, instead of proceeding from the equator to the pole and back again, go on the north side from the pole to the obstruction, and back to the pole; while on the south side, the flow is from the equator towards the obstruction, and back again towards the equator, leaving the obstruction a dividing line marking great difference of climate in the winter season.

In the New World a ridge of mountains extends from Mexico by the Rocky Mountains, some of which are stated to be 25,000 feet high, to the Frozen Ocean. This ridge crosses the diagonal line of the great atmospherical currents, and constitutes such an obstruction as that described. In the Old World, a number of similar ridges extend from the southern point of the Himalaya Mountains to the Swiss Alps, including the range of the Himalaya, Hindoo Koosh, Central Asia, Armenia, Circassia, the Carpathian Mountains, and the Illyrian and Swiss Alps; and the climates found to the north-east of these chains are materially different from those which exist to the south-west. The greatest difference in climate in those parts is found in the beginning of winter, and is, it is presumed, caused by the different quantities of atmospheric steam condensed in the respective parts. In the tropical seas, a quantity of steam exists in the atmosphere sufficient to give a dew point of 80°, making the steam 1-48th part of the whole atmosphere. This steam, if all condensed into water, would give a depth of about nine inches. The steam is regularly carried, in the autumn and the beginning of the winter, when the northern hemisphere is cooled down, from the tropical regions in a north-east direction towards the polar regions, or towards some obstructing elevation of the land, and is to a great extent condensed; and it is to the condensation of this steam that we are to look for the great difference of winter climate in the same latitudes of the northern hemisphere. The steam in the tropical regions of the Pacific Ocean that flows towards the north-east, with the south and south-west winds that prevail in those parts, is carried to the American ridge, and is there condensed. The result is, that the south-west side of this chain of mountains is wet and warm in the winter, from the tropics to Nootka Sound, and still farther north. Captain Cook, Lewis and Clarke, Captain B. Hall, and Humboldt, describe the climate of this part in such way as can leave no doubt of the fact. But beyond this ridge, to the north-east, we have a different climate in the winter, it being as remarkable for being cold and dry, as the other side is for being wet and warm. Captain Parry, Captain Back, and Lewis and Clarke, represent the country in the winter, from the shores of the Frozen Sea to the Missouri, as very cold, and generally dry. Here we trace the effect of the condensation of steam, and of its absence, on the climates of the different parts. In the Old World the same causes produce the *same* effects. On the south-west sides of the various ridges of mountains, the weather is in the autumn and early part of winter very wet and warm for the latitudes. This is particularly seen in Hindostan and the south-west coast of Italy; while to the north-east of these mountains the climate is cold and dry, extending over Poland, Russia, Central Asia, and Siberia. The very heavy rains which fall to the south of the Himalaya Mountains indicate the great condensation of steam that takes place in that part of the world; and the effect produced on the climate is remarkable. The valleys are habitable to a great elevation, and Major Archer states, that wheat is grown at a height of 13,000 feet, in lat. 32° north; whilst Humboldt represents 1300 feet as the greatest height at which wheat can be grown in Teneriffe, a place 4° more south. But when the steam that is in the atmosphere is all, or nearly all, condensed against the sides of elevated ridges, it is evident that it cannot carry its warming influence farther north. Hence the part of the globe between these ridges and the polar regions will, in autumn and winter, be dry and very cold. In order to reason on the causes of the difference of winter climates in these latitudes, let it be supposed that the present Asiatic mountains were removed further north—say to Northern Siberia. Then, as there would be no longer elevated land to intercept and condense the steam, where it is now condensed, it would flow farther north, towards Siberia, and be there condensed, and it would render that country, now so dry and cold in the winter, wet and warm. The British Islands are now made warm in the autumn and the beginning of winter by the steam that is brought from the tropical regions. But suppose a lofty ridge of mountains to extend from the Canary Islands to New York, and the effect would be, that the steam now brought north to more than 50° of latitude, would be condensed 10° to 15° more south, and the British Islands would be as cold in the winter as the same latitudes are at present in Asia and America. That the relative situations of land and water are not the cause of the great difference of climate, may be shewn by supposing an alteration in certain parts of the earth's surface. Were central Asia to become sea instead of land, that circumstance would not prevent the present mountains from condensing the tropical steam, as they now do, and consequently would not prevent that degree of cold and dryness which results from the interception of that steam in those parts, though they would be then sea instead of land. In like manner, suppose a belt of low level land to extend from Spain across to Davis' Straits, which is now sea; and it will be perceived that this part, being land instead of sea, would not, in the summer and autumn, prevent the tropical steam from flowing to the British islands. It would not be until winter had cooled down this supposed land below the temperature of the present sea, that any greater condensation of tropical steam would take place south of the British islands. From these various considerations, we are warranted in coming to the conclusion that the great difference in the winter climates of certain parts of the northern hemisphere, is attributable to elevations of land intercepting and condensing atmospheric steam, and thus rendering those parts wet and warm, while, cutting off the supply from more northern parts, leaves those parts dry and cold.—*Athenæum, No. 720, page* 624.

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## *Report on the Bude Light.* By Andrew Ure, M.D., F.R.S.

From the Report of a Committee of the House of Commons, it appears that this light is so called from *Bude*, in *Cornwall*, the residence of its inventor, Mr Gurney—a name bestowed upon it at the Trinity House, to distinguish it from the ignited lime light which he first described in his work on chemistry in the year 1823.

The Bude light originally consisted of an oil argand flame, having a stream of oxygen thrown up over its internal surface, which produced a very vivid illumination. It was found, however, after having been used for some time in lighting the House of Commons, that oil lamps thus fed with vital air were expensive and difficult to regulate.

Mr Gurney then tried to illuminate the House with naphthalized coal-gas, in argand burners, similarly supplied with oxygen; and though this produced a light of sufficient intensity, he encountered a formidable obstacle to its continuance from the deposition of liquid naphtha in the tubes of distribution. He next happily discovered a method of obtaining, from ordinary coal-gas, purified in a simple apparatus of his own, and burned with oxygen derived from the atmosphere, an effulgence adequate to every purpose of internal and external illumination, which is now used in the House of Commons with perfect success, and at a cost of only twelve shillings per night, whereas that of the candles previously used there amounted to six pounds eleven shillings per night.

This new Bude light possesses the following advantages over all other kinds of artificial illumination hitherto displayed.

*First.*—It gives as much light as the best argand gas flames, with only one half the expenditure of gas. This very remarkable fact was established by experiments carefully conducted with the same standard wax candles which I employed for comparison prior to my examination before the late committee appointed to ascertain the best mode of lighting the House of Commons. A common argand gas flame was found to emit a light equal to ten such candles (three to the pound); and a Bude burner, called No. 10, gave a light equal to 94.7 of the candles. Thus, the Bude flame had nearly ten times the illuminating power of the gas argand flame; while, by means of an accurate gas-metre, the former was ascertained to consume only 4.4 times the quantity of gas consumed by the latter, demonstrating the economy of the Bude light over common gas to be greater than two to one; and this economy increases in proportion to the magnitude of the light. The source of this surprising superiority may be observed by comparing the two flames: the base of the argand gas flame is of a blue tint for fourteen-sixteenths of an inch, a space in which the gas burns with intense heat, but little or no light; whereas the base of the Bude flame acquires a dazzling whiteness at three-sixteenths of an inch from the metal. Thus we see, that, through a range of eleven-sixteenths of an inch, the common gas argand flame is wasted in producing the nuisance of heat without light.

*Secondly.*—From the phenomena just noticed, as also from the circumstance of the Bude flame emitting a double light with a single volume of gas, when compared with the gas argand, it is manifest that the former, in equal degree, can disengage at the utmost only half the heat that the latter does.

*Thirdly.*—The Bude light simplifies greatly the means of artificial illumination, since it concentrates in one flame as much light as will diffuse, throughout a large apartment, a mid-day lustre, which may be softened by shades of every hue, and reflected by mirrors in every direction.

*Fourthly.*—From this property proceeds its value as a ventilator, since the single tube which carries off the burned gases serves to draw out also the effluvia from a crowded chamber.

From all these facts, I am of opinion that Mr Gurney's new Bude light is a most meritorious invention in reference to both public and private buildings, as it removes altogether the objections hitherto justly urged against the use of the highly hydrogenous gas of the London companies in dwelling-houses, namely, that its heat is great in proportion to its light, when compared with the more highly carburetted gases of Edinburgh and Glasgow.

The time must therefore be now at hand when the great economy and convenience of lighting private houses with gas will be experienced by the inhabitants of the metropolis, as they have been for such a considerable time by those of every town of importance in Scotland.

That the same quantity of coal-gas may be made to produce a double amount of illumination in Mr Gurney's patent burner to that obtained from it in an ordinary argand, will appear to many a paradoxical, if not a doubtful, proposition. Of its reality, however, I am fully convinced, and I think the fact may be accounted for in the following way:—

Light, in general, is proportional to the intensity of ignition, a truth well exemplified in the effect of the oxy-hydrogen flame upon a bit of lime or clay. On the same principle, when the flames of two candles are brought into close contact, they afford a compound light considerably greater than the sum of their separate lights. Now, Mr Gurney's burner gives such a compound flame. It consists of two or more concentric cylinders of flame, mutually enhancing each other's temperature, just as in Fresnel's polycycle oil argand lamps used in the French lighthouses.

In addition to the augmented intensity of ignition, we must also take into account the peculiar nature of the combustion of carburetted hydrogen gas, whether as generated from coal in a retort, or from oil in a lamp. T[illegible]d whiteness of its flame is due to the separation in solid particles, and subsequent ignition of its carbon. Pure hydrogen, when burned, affords a very feeble light; and whenever so much air is mixed with coal-gas as is sufficient to consume all its carbon simultaneously with its hydrogen, it burns with a dim blue flame. Now, in the base of a common argand flame, an excess of cold atmospheric oxygen is allowed to act upon the coal gas in the vacant spaces between the pin-holes, whereby the temperature being greatly lowered, while the carbon is consumed in the gaseous state, the light from these two causes is nearly null. It is not till the gaseous mixture rises and forms a continuous hot cylinder, without interstitial streams of air, that it emits a white light from the ignited particles of the carbon precipitated in the interior of the flame.

In Mr Gurney's concentric series, the prejudicial excess of atmospheric air is prevented, and only so much permitted to come into contact with the gas, as will effect the due separation and ignition of its carbon, even at the origin of the flame.

To these two causes conjoined, viz. the increased intensity of ignition, and the limited supply of oxygen, it is that the new Bude flame owes its economy of illumination. The effect of oxygen in excess, is elegantly demonstrated by throwing up a stream of it within a gas argand flame, for the light is thus nearly annihilated, while the heat is prodigiously augmented.

As regards the specification of the patent for this improved mode of lighting, which I have carefully examined, I have no hesitation in declaring it, in my opinion, to be valid and unimpeachable.—*The London Journal and Repertory of Arts, Sciences, and Manufactures,* No. CXXV., p. 292.

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## *Remarks on the Climate of Egypt.* By M. Joseph Russegger, Austrian Councillor of Mines.

*Seasons.*—Lower Egypt, lying between the 30th and 31st degrees of latitude, belongs, in so far as regards the yearly periodical sequence of the seasons, to the system of Southern Europe, but of course presents those modifications which are peculiar to warmer climates. Thus, Lower Egypt has its summer and its winter at the time of our own, only with this difference, that the latter is a period of rain, which occurs during our winter months. Spring and autumn almost entirely disappear in warm climates, and there those delightful transitions from winter to summer, and *vice versa,* which we enjoy in the more temperate zones, are unknown. In tropical countries there is nothing but summer, viz. a summer which is entirely dry, and one during which it rains more or less. These rains continue there during half the year; whereas in *Egypt, as in a northern* winter, they are confined to a few *months. Of course they* do not give rise to that winter-sleep of the organic creation, especially of plants, which characterises our northern winter, but rather raise the whole vegetable kingdom to the highest pitch of its vital development, and to the fullest expansion of beauty. Thus Egypt is, in fact, never more attractive than at the period when nature is with us covered with snow and ice. This alternation of the two principal seasons of the year, the summer and winter, represented by the dry period of the year and the rainy, is, however, in the order in which we have them in Europe, peculiar to the coast region of Northern Africa; and there begins in Egypt, south of the 30th degree of latitude, that remarkable zone which extends to the 18th degree, therefore over 12 degrees, and which, owing to the rare atmospherical precipitations of water that occur within its limits, I term the zone of little rain (*die regen-arme zone*). Within the limits of this zone is placed the African region of deserts, which, where the tropical rains begin (which rains, to the north of the Equator, fall during our summer), viz. to the south of the 18th degree, again gives place to the Savannah-region, to those districts so remarkable for their fertility on the banks of the great rivers.*

We not unfrequently find it boldly asserted in the accounts given by travellers of the climate of Lower Egypt, that it does not rain at Cairo. This statement is untrue, and is one for which science has to thank the presumptuous conclusions formed by the ignorant or the credulous. In Egypt and Nubia there is no district devoid of rain—at least for the natural philosopher; the peasants, it is true, not satisfied with a few drops, judge otherwise, and their ideas require much correction to bring them to the truth. There are, however, districts where it rains very rarely, and, even among these, Cairo, with its vicinity, is not to be reckoned, for year after year storms take place there during our winter months, which rarely pass away without rain. Just as the years 1761 and 1762, in which Niebuhr made his observations at Cairo,† undoubtedly belong to those which are remarkable for the quantity of atmospherical precipitations, and ought, therefore, to be included among the exceptions; so, on the other hand, there are years when these precipitations are particularly rare, and which, therefore, are also to be regarded as exceptions. We cannot employ either extreme as a guide to enable us to deduce the mean yearly quantity of rain, and conclusions exclusively confined to the one or the other are hence incorrect. The yearly quantities of rain increase by gradations southwards from Cairo towards the tropic, although, however, in the vicinity of the great river, that is, in the actual valley of the Nile, the rain is more observed than in the deserts on the two sides of the stream. Northwards from Cairo, on the other hand, the phenomena of the true littoral climate extend not only over the Delta, but likewise eastwards and westwards into the deserts, where Ehrenberg, Hemprich, and General Minntoli, suffered not a little from violent rains during their journey to the oasis of Jupiter Ammon.* At an earlier period, this climate, which now belongs to Lower Egypt as a littoral region, would seem to have stretched more to the south; for we find in Upper Egypt, as well in the valleys of the Arabian as of the Libyan mountains, that is, of the mountains eastwards and westwards of the Nile, the most evident traces of violent falls of rain, viz. many dried up beds of torrents, which, as appears from the boulders transported by them, and from the considerable ravines which they have furrowed, must have been deep and powerful.

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* See my Essay on the Meteorology and Climate of the African Tropical Region in Dr Holger's *Zeitschrift für Physik und verwandte Wissenschaften*, vol. vi. part 2. Vienna, 1840; and Contributions to the Physiognomy and Geography of the African Tropical Region in Leonhard's *Jahrbuch*, 1840.

[illegible]*en Niebuhr's Reisebeschreibung nach Arabien*, &c. Kopenhagen, 1774.

*Barometer.*—The conclusion derived from horary observations of the barometer is, that in Egypt, as in every place where I have been able to make observations in Africa and Asia, the pressure of the atmosphere attains twice a-day a maximum and twice a-day a minimum. The maxima occur at 10 A.M. and 10 P.M., the minima between 4 and 5 P.M. and a little before sunrise, corresponding with the minimum of the daily temperature. Although the differences of the extremes at night are sometimes very small, always much smaller than those of the day, yet the case occurs but rarely of their being imperceptible to sharp observation and with first-rate instruments.

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* Journey to the Temple of Jupiter Ammon in the Libyan desert, by *Baron von Minutoli, Berlin, 1824*; and Travels in Egypt, Libya, Nubia, and *Dongola, by Hemprich and* Ehrenberg. 1. vol. 1. part. Berlin, 1828.

*Temperature.*—The daily temperature of Lower Egypt presents only two extremes, one maximum and one minimum; the former of which occurs between 2 and 3 P.M., and the latter a short time before the rising of the sun. Egypt is among the hottest of those countries of the globe which lie without the tropics, but this applies properly only to Upper Egypt; for Lower Egypt, as a littoral tract, is too much exposed to the cooling action of the sea-breeze not to have a diminished temperature. In Alexandria and on the Delta the temperature rarely attains 30° R. (99°.5 F.); but in Cairo, which is not exposed to the sea-breeze, and has deserts on both sides, the temperature is much higher, and often attains 30° R. and above it, in the complete shade of a perfectly opaque body. The mean temperature of Lower Egypt is between 17° and 18° R. (70°.2—72°.5 F.). As at nightfall the wind is generally from the north, the nights are perceptibly cool in comparison with the temperature of the day, and there arise differences between the temperature of the day and that of the night of from 10° to 12° R. (22°.5 to 27° F.), which, it is true, are inconsiderable when compared with the differences of temperature of night and day in the equatorial regions of Africa, but are very great in comparison to the same phenomena in Europe. During a prevalence of north winds, and the greatly diminished temperature which accompanies them, and after a deposition of dew, which appears more especially when the wind is north, it often happens, particularly on the extensive plains of the desert, that the thin covering of moisture which lies on the surface in the morning becomes frozen, and thus we have in a very simple way the phenomenon of the formation of ice in the deserts of Africa. If in such a case the temperature of the atmosphere be not so low as that a freezing of the dew should be directly produced by it, yet this takes place in consequence of the diminution of temperature in the liquid layer and in the immediately adjoining air itself, and of the rapid evaporation which results from the sudden change of the strata
ir, caused by the prevailing north wind; and thus we
*formation* of ice, not only in the deserts of Egypt,

but even, though more rarely, in the deserts of the interior of Africa.

As the temperature is lowered by the north wind on the one hand, so, on the other, it is raised by the south wind, to which latter also, in respect of its direction, the *Chamsin* belongs. This elevation of temperature is not inconsiderable. and amounts to several degrees of Reaumur, so that the thermometer rises at Cairo to considerably above 30° R. (99°.5 F.); but it is not so great as some have asserted, at least in southern latitudes, where I observed the *Chamsin* wind long and carefully, and where the phenomenon is more powerfully displayed than in Egypt. Sometimes the south winds of Egypt lower the temperature just like the north winds, and this takes place when early and very violent periodical rains occur in the tropical regions.

In reference to the observations on the temperature of the air in the shade and in the sun, which I made at Cairo and Alexandria in the month of April, I have drawn up tables* in which are also introduced the direction of the wind, the nature of the clouds, and the state of the weather. These tables do not give us the laws of the daily range of temperature, as they want observations at the time of the minimum, and afford too few terms for accurate calculation. Observations free from these deficiencies were made by me during a subsequent residence in Egypt, and will afterwards enable us to determine these laws. The tables, however, of which I now speak, shew us pretty nearly the maximum of the daily temperature, and its fall on both sides towards the period of minimum.

The highest temperature observed in the shade at Cairo in the month of April was 27°.3 R. (93°.4 F.), and in the sun 31°.8 R. (103.6 F.); the lowest in the shade 14°.1 R. (about 64° F.), and in the sun 19° R. (74°.7 F.); the differences observed, therefore, = 13°.2 R. (about 29°.4 F.), and 12°.8 R. (about 28°.9 F.) These differences, however, are not those of the extremes, as we want the observations on the minimum.

In Alexandria the highest temperature observed in the shade in the month of April was 20° R. (77° F.), and in the sun 28°.2 R. (95°.4 F.); the lowest in the shade 14°.3 R. (about 64° F.), and in the sun 23° R. (83°.7 F.); and therefore the differences = 5°.7 R. (about 13° F.), and 5°.2 R. (about 11°.7 F.); therefore much smaller than at Cairo, which accidentally harmonizes with the law according to which these differences increase as we approach the equator, while the differences of the extremes of the pressure of the atmosphere diminish.

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* Page 211 of the first part of the "*Reisen.*"

If we take the mean of all the observations in the shade, about three hours after the minimum, we have,

| For Cairo, | | For Alexandria, |
|---|---|---|
| 16°.7 R. (69°.6 F.) | . . . . . . | 16°.9 R. (70° F.); |

and in the same way the mean of all the observations at 2 P. M., at the time of the maximum, we have,

| For Cairo, | | For Alexandria, |
|---|---|---|
| 23°.1 R. (about 84° F.) | . . . . . . | 18°.5 R (73°.6 F.) |

therefore the differences are

| For Cairo, | | For Alexandria, |
|---|---|---|
| 6°.4 R. (14°.4 F.) | . . . . . . | 1°.6 R (3°.6 F.) |

The mean of the means of the two series of observations made at the same hour for April are thus:

| For Cairo, | | For Alexandria, |
|---|---|---|
| 19°.9 R. (76°.7 F.) | . . . . . . | 17°.7 R. 71°.8 F.) |

If we consider the observations made on temperature in Lower Egypt during a whole year, we find that the months of July and August are those which present the highest elevation of the thermometer, and the month of February that which is characterized by the lowest temperature. This is an arrangement of temperature which reminds us completely of Europe.

Niebuhr observed the lowest temperature in Cairo in February in the morning, and it was 42° F.; and the highest in June and July, when it was 101° F.; the first having been during a south wind, and the second during a north wind. The difference of these extremes is 59° F., being the range of temperature in a period of eight or ten months; the mean of these extremes is 71°.5 F., which approaches nearly the mean temperature assigned to Lower Egypt.

From Niebuhr's whole observations we find that the arithmetical mean of the yearly averages of the two extremes = 17°.82 R. (about 72° F.), which corresponds nearly with the arithmetical mean of the whole observations, viz., 17°.25 R. (70°.7 F.) We have, then, these two numbers, but especially the first, viz., 72° F., which may be regarded as the mean temperature of Cairo.

*Winds.*—In Lower Egypt, during the whole year, the wind blows from the N. N.E. and NW., with a little interruption of E. and W. winds; and it is only in the months of April and May that south winds occur. In the tropical regions, again, in 15° lat., the north winds blow nearly six months, viz., during a portion of November, and in December, January, February, and March; whereas, during the rest of the year, south winds blow almost uninterruptedly, which advance towards the north from the equator along with the southern tropical rains.

*Chamsin and Simum winds.*—The *Chamsin** occurs during the period of the south winds, that is, in the months of April and May. This wind has its name from Chamsin, which means fifty, from the Arabians asserting that it blows repeatedly, exclusively during a period of fifty days. It is frequently confounded with the *Samum*, from which, however, it is essentially distinct. The Chamsin is a periodical, yearly recurring wind, which always comes from the south and south-east, more rarely from the south-west; and its cause and its whole phenomena appear to be of electrical origin. The Samum, on the other hand, is an ordinary storm from the desert, which has no fixed period of occurrence, and has no particular direction, but comes from entirely opposite quarters. It is rendered formidable by its heat, by its violence as a storm, and by the quantity of sand and dust it brings along with it. The danger which is combined with the Chamsin is quite of a different description from that of a hot storm carrying sand with it; frequently it is not at all a storm. Its alarming feature is an action peculiar to itself, which operates in a positively hurtful way on the body, and which probably depends on an extraordinary accumulation of electricity in the air. If the Samum* be violent, it is, as a wind of the deserts, really perilous for caravans, because, passing over the burning sand, it becomes intensely heated, to such a degree as to be almost unbearable, and also from the masses of sand and dust which it transports along with it, and heaps up into hills. Animals are rendered wild, and throw off their burdens; while men lose their presence of mind, and, just as happens in violent snow-storms on high mountains, becoming exhausted, sink under the contest with the heat, the sand, and the storm. The chamsin is rarely a storm of long duration, and its most violent period is soon over; but the atmosphere remains for a long time extremely hot, so that in the shade the temperature reaches 40° R. (122° F.), though I myself have never seen it above 38° R. (117°. 5 F.); the air is filled with extremely fine sand and dust, which penetrate everywhere, and against which no covering, no window, affords protection; breathing is rendered difficult; the blood flows to the head, and individuals of a full habit of body, or whose nervous system is affected and weakened, are in danger of dying from apoplexy. Such cases, however, are not of frequent occurrence, and are rarer than is generally asserted. Chamsins generally follow oppressive heats, and the air is always unusually dry.

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* The Ch is pronounced sharply like the x in Spanish, in the words Mexico, Quixote, &c.

Far towards the horizon, and chiefly in the south-east, there appear thick black clouds, to which fire-red clouds succeed, and form a mass with the first, exactly like the burning clouds rising from the conflagration of a large town. A dingy reddish-yellow light spreads itself widely, an oppressive heat is experienced, a calm prevails, a painful silence pervades the whole of nature, and men as well as animals seek shelter. A dull hollow sound is heard, the clouds rolling onwards arrive, and in a moment the storm takes place; everything is enveloped in a sea of sand and dust, against which shelter scarcely affords protection. In Egypt, these chamsins generally terminate without a fall of rain, but this is not the case in more southern latitudes; and there all the phenomena of this kind of wind are more distinctly and characteristically exhibited. I had frequent opportunities of observing the whole course of the chamsins in the deserts of Southern Nubia, and in the immeasurable grassy plains of Kordofan and the White River; but upon this subject I shall speak more in detail in a subsequent part of my travels. I now merely repeat, that the chamsin has nothing at all in common with the ordinary winds which owe their origin to the disturbance of the equilibrium of the strata of the air, caused by purely mechanical means; but that it is entirely an electrical wind, as well in its origin as in its whole course.*

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* Samum is perhaps a Turkish corruption of the Arabian word Semen, poison, by which word the Arabians also often designate the action of the Chamsin, whence probably the frequent confounding of the two words, Samum and Chamsin.

*Inundations of the Nile, and Vegetation.*—I have already remarked, that the climate of Lower Egypt belongs to the type of Southern Europe, only with those modifications which are peculiar to warmer zones. We have, as there, on the coasts, the violent winter storms from the north, and especially from the north-west; storms at the periods of the Equinox; rain in the months of November, December, and January; frequently at Alexandria, more rarely at Cairo, and almost exclusively after thunder-storms. With this exception, the sky is nearly always clear and bright, the air during the day dry, and at night moist; so that during the summer months there is much dew every morning. Except during the winter months, it rains rarely in Alexandria, and for the most part not at all at Cairo. The rising of the Nile is merely a consequence of the tropical rainy period, and that not merely in so far as regards Abyssinia, but also in so far as relates to all the countries which send their waters to the river district of the Nile, and its two great branches, the Blue and the White Rivers. Neither the melting of the snow, nor the falling of it on the high mountains of Abyssinia, comes here into consideration, for the snow is of no consequence whatever, and its operation on the height of the Nile one of those illusions called forth by absurd hypotheses, which one person after another repeats, sometimes during whole centuries, with all the accompaniments of pedantry. Whoever has witnessed one or several rainy periods of the interior of the tropical regions of Africa, can very well understand their influence on the rising of the river. In Lower Egypt, the rise of the Nile is first observed in the month of June, and in the month of September the river reaches its highest level. At that time its broad bed is entirely full, and the neighbouring banks are here and there covered with water. The country, however, has by no means the aspect of a large lake, for the water is everywhere restrained by dykes, and extends only in canals, so that the communication for foot passengers and horsemen between villages and towns is rarely cut off. At the end of September, the river begins to fall, and in October and November the cultivation is commenced of the portions of ground which had been irrigated by water admitted from the canals. The fruitfulness of the soil, caused by this flooding and artificial irrigation, is not only comparable to that of the most fortunate countries of the globe, but even surpasses the greater number of them. It is, however, confined to that portion which the river itself has created, and over which it yearly distributes the blessings of its floods; all the rest of the country is a desert. In the months of October and November, when the water of the Nile has retired and deposited its mud, the first sowing of grain takes place, and the crops are reaped so early as February and March. In April grain crops are sown for the second time, and the harvest arrives before the succeeding rise of the river. In the intervening period the harvest occurs of the December and January grains of other fields. After the inundation the sowing of cotton takes place. After three years, although the plant lives and is productive for a longer period, the cotton seed is renewed, and it is always the practice to have perfectly fresh and strong plants. The irrigation of the cotton is entirely artificial, for the plants must not be exposed to the inundation. This irrigation takes place in winter at intervals of from 12 to 14 days, and in summer at intervals of 8 days. The plant is productive the first year, and the harvest occurs in the month of July, from which time till winter it is continued. The produce of a healthy plant amounts to two pounds yearly. At the time of the first sowing of grain in

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* See my Memoir on the Climate of the Tropical Regions of Africa, already quoted.

Egypt the fruits of various trees are ripe. In January are sown beans, lupines, and flax, which are gathered in the first part of summer. In February the rice is sown, whose harvest happens in September, in which month also the orange, lemon, and olive-trees yield their fruit. In January the sugar cane is cut in Egypt. In May, grapes, figs, and Carob-beans ripen. Clover is cut three times in the year. Thus, in this rich country, there is no month in which nature does not produce flowers and fruits. What could not such a country become in the hands of a wise government, one which would truly and judiciously promote the industry and welfare of the people? What prosperity might be developed, and at present what misery prevails! The climate of Lower Egypt is to be regarded as of the happiest description, for it favours to the utmost the cultivation of all the vegetable productions of Southern Europe, while it admits of the growth of most of those belonging to the warm tropical regions. I think that the following tabular view of the seed-time and harvest occurring in each month may not be uninteresting as regards the cultivation of Lower Egypt. The data are derived partly from my own observations and partly from those of other travellers.*

| Month. | Sowing. | Harvest. |
|---|---|---|
| January, | Lupines, Beans, Flax. | Sugar-cane (in Upper Egypt in June), Senna, Clover. |
| February, | Rice, Maize, Millet. | Barley, Colewort, Cucumbers, Melons. |
| March, | Cotton. | Grains, Maize, Millet of previous autumn. |
| April, | Grains, Cotton. | Roses, Clover. |
| May, | | Winter Grains, Grapes, Figs, Carob-bean, Saffron, Dates as early fruit. |
| June, | | Saffron, Lupines, Beans. |
| July, | Planting of Rice, Maize, and Millet. | |
| August, | | Clover. |
| September, | | Rice, Oranges, Lemons, Tamarinds, Olives. |
| October, | Grains, Maize, Millet. | Rice, Meadow Grasses, Pomegranates. |
| November, | Grains, Vegetables. | Dates, Maize, Millet (of February). |
| December, | | Meadow Grasses, blossom-period of early flowers. |

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* Prokesch, Erinnerungen aus Egypten. Champollion-Figeac, Beschreibung von Egypten, Stuttgart, 1841. Niebuhr, Reisebeschreibung nach Arabien.

Although the soil of Egypt, which is entirely mud of the Nile, is thus productive, so long as it is cultivated and watered, it is singular how speedily it is converted into desert, whenever it is neglected by man. Salts are formed, and especially saltpetre; the rich soil becomes rapidly parched, and falls into dust, which becomes the sport of the winds; and no vegetation takes root on the very spots which by the smallest care would become remarkable for their fruitfulness. (From the 1st Part of Russegger's *Reisen in Europa, Asien, and Africa.* 1841.)

## *Essay on the Glaciers and the Erratic Formation of the Basin of the Rhone.* By Jean de Charpentier.*

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In science, as at table, "tarde venientibus ossa:" it is of consequence to a scientific man that he should not allow himself to be anticipated in the publication of his researches or discoveries. The book we now announce is the fruit of many years' labour, travel, and observation on glaciers and erratic deposits; it is only the development of ideas which the author had summarily promulgated in a memoir in 1834; it is full of interesting and curious facts; but it comes in the train of many works of the same kind, which have appeared in 1840, and, although the authors of these works have all scrupulously acknowleged M. de Charpentier's merits in the matter, it must be deficient more or less in the attractions of novelty. In the February number of the *Bibliothéque Universelle de Genève* for 1841, an account has been given of the works of MM. Godeffroi, Agassiz, and Rendu, on the glaciers of Switzerland and Savoy, and the general facts resulting from the phenomena presented by glaciers are noticed in sufficient detail. We may confine ourselves therefore, in this part of the subject, to a brief notice of the principal points in the history of glaciers, regarding which, M. Charpentier's opinion differs from that of other geologists. The theories on the origin and development of glaciers are still matter of controversy, and the great influence which it is now thought may be ascribed to them in all that relates to the dispersion and transportation of erratic blocks, gives a new interest to the examination of all the facts connected with them.

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* Essai sur les Glaciers et sur le terrain erratique du bassin du Rhone, par J. de Charpentier. Lausanne, 1841, 1 vol. in 8vo.

According to M. de Charpentier, it never snows in flakes on high mountains, on account of the dryness of the air; but the vapours condense in such situations in transparent rounded grains, similar to those we call hoar-frost. It is this which constitutes the upper névé of the Alps, and which is transformed into glacier at its lower portion. The snows of the equatorial Cordilleras are all névés, and their coherence is so slight, that they resemble a mass of ashes, which the wind carries off in whirls.

In order to account for the conversion of the névé into glacial ice, M. de Charpentier maintains the same theory admitted by Agassiz, that is, the absorption and congelation of the rain-water, or that which proceeds from the melting of the névé, uniting and cementing the grains. This absorption of water takes place unequally according to the height, inclination, the vicinity of crevices, &c. It is during the day, in summer, that the inbibition of water takes place, and it is during the night that such water becomes congealed. The expansion of the ice formed, and the unequal distribution of the water in the glacier, cause a tension which ruptures and splits the whole mass of the glacier, and these capillary fissures extend in every direction. M. Agassiz ascribes the fissures to the compression of the bubbles of air enclosed in the ice, but M. de Charpentier's explanation appears the most probable. These innumerable fissures render the glacier porous and permeable to the water which the heat of the following day will produce, and this alternate freezing and melting continue throughout the summer. Thus we hear in glaciers during the night the cracking noises produced by the rupture of the ice, sounds sometimes so loud that they might be taken for the reports of a cannon.

M. de Charpentier does not admit that the ice of glaciers is stratified, as M. Agassiz seems to believe; he thinks that stratification cannot appear but in the elevated névés, where the annual snows are not completely melted,—never in the glaciers properly so called.

With regard to the descending progress of glaciers, M. de Charpentier ascribes it to the effect of the dilatation of the water congealed, during the nights of summer, in the interior of the glacier, and which tends to push the mass in the direction which presents least resistance, that is to say, in the direction of its length. This effect, which is of such a nature as to be continually renewed, would cause an unlimited increase in the glacier, if, after its arrival in the lowest valleys, the glacier were not exposed to a higher temperature, which destroys it by fusion. According as the expansion and the fusion counterbalance each other, or the one gains an influence over the other, the glacier remains stationary, advances or diminishes. M. de Charpentier endeavours to establish this as the true theory of the movement of glaciers, on the grounds, 1*st*, That the motion never takes place except in summer, the season of the alternate melting and congelation of the water in the day and night, and that the glaciers are stationary during winter; 2*d*, That the glaciers always advance, when a cold and rainy summer succeeds a winter in which there has been much snow, as was the case from 1812 to 1818, and that they diminish when the summers are dry and warm, and the snow not in great abundance, as from 1821 to 1826; 3*d* and *lastly*, That the small inclination of many glaciers, and their stationary condition during winter, prevents us ascribing their motion to the pressure of the snow in their upper portion, as Saussure supposed.

This expansion of the ice produces an enormous pressure on the rocks which form the bed of the glacier. The friction resulting from the movement of the mass, aided by the sand or gravel occurring between it and the rock, hollows, ruts, or polishes the latter, according as the minerals constituting the rock are of a more or less hard or resisting nature. The fine striæ formed in this case always follow the direction of the glacier's progress,—a direction which is determined by the configuration of the ground.

From a desire to avoid all useless repetition, we do not
fer to the definitions and the mode of formation of

moraines. M. de Charpentier has studied this part of the subject with much care; he has distinguished the forms which these accumulations of blocks assume, according as the glacier advances, remains stationary, or diminishes; according as they are formed against an escarpment, or on the height of a precipice, &c. He invariably distinguishes between a fallen mass of rocks and a moraine, by the former containing only one kind of blocks, while moraines are composed of angular and rounded blocks, and such as are rubbed by the movement of the ice, including at the same time various species or varieties of the rocks entering into the construction of the mountains which overhang the glacier.

The phenomena belonging to superficial moraines, to their pedestal of ice (which they form by preventing the fusion of that part of the glacier which they cover), to the dislocation and scattering they undergo when the glacier which bears them enlarges or melts, are all treated in detail in M. de Charpentier's work. He likewise particularly notices a peculiar modification of moraines which he calls *alluvium glaciaire.* This is when the debris of rocks carried by glaciers, instead of falling or being heaped up on a dry deposit, fall into reservoirs of water. They then form irregularly stratified deposits, which are distinguished from ordinary alluviums by the size and shape of the blocks, and by the want of that polish which characterises the pebbles of transported rocks. He mentions a great many instances of this in existing glaciers, and many others at the mouth of the valleys opening into the great valley of the Rhone, and in localities where the glacier which formed the bounding wall of the reservoir of water has long since disappeared. He denominates such depositions as these, the *diluvium glaciaire.*

M. de Charpentier does not admit the explanation given by M. Agassiz of the return to the surface of the glacier of the blocks which have fallen into its interior. He conceives that this result, which is otherwise well established, is solely owing to the gradual melting of all the ice which covers them; but he cannot believe that there is a real rejection of a foreign body by the ice, by reason of the pressure exercised by the congealed water around its sides, as the distinguished naturalist of Neufchatel seems to suppose. If the phenomenon is not observable in the elevated névés, this is solely, in M. de Charpentier's opinion, because the melting of the annual snows is not completely effected in such places.

With regard to the crevices so common in glaciers, M. de Charpentier ascribes their origin either to the unequal expansion of the ice, when the congelation of water takes place in the interior of the glacier, or to the dislocations produced by the inclination of the bed of the glacier, or to the inequalities of that same bed, &c. When these rents are formed,—and that is usually accompanied with a loud noise,—their width rarely extends to 5 lines, and never exceeds these dimensions; but if they communicate with the lower face of the glacier, either directly or by the intervention of other rents, they speedily enlarge by the melting of the ice on their sides, occasioned by the access of the air or rain-water, until they sometimes acquire a width of 10 feet. When the crevice does not extend to the bottom of the glacier, it becomes filled with water at its termination, and that freezes in winter. If this ice does not continue compact like that of rivers, it is because it is soon subjected to the influence of the expansion of the ice formed in the night during summer, when it becomes rent and granular like that of a glacier. These crevices distribute the water produced by rain or melting throughout the whole extent of the glacier, and bring it in contact with the capillary fissures, which absorb it in greater or less quantities; the rest follows the bed of the glacier, and issues from it in the form of a torrent. These waters are always mingled with a very fine mud, which appears to be the produce of the rocks ground by the friction of the glacier. Accordingly, the waters greatly diminish on the arrival of winter, and are then perfectly clear. It is probable that, if the torrents which flow from glaciers are not completely dried up during winter, it is owing to the existence of springs under the glacier, which issue there from the interior of the earth, and not because the glacier melts, even in winter, on its lower surface. In fact, the observation made on the glacier of Gétroz by M. Venets, and some other facts cited by M. de Charpentier, tend to induce the belief that the underside of glaciers, on the contrary, is always frozen, even during the heats of summer.

After some brief observations on the ice of northern regions, which he divides into floating and terrestrial ice, the former proceeding from true glaciers produced by the congelation of the névé, and the others from ordinary congealed water, M. de Charpentier arrives at the second part of his work, which treats of the *erratic formation.*

Although it may perhaps be difficult to give a good definition of this formation, without confounding it with the diluvial or alluvial soils, it is so well known to geologists under the name of erratic blocks, that there is no need of characterising it by a description. The size of the blocks, which in no degree diminish in proportion as we recede from the places where they originate, as takes place in regard to the diluvial formation, the rough surface and sharp angles of many of these blocks, the almost invariable want of selection according to size and stratification, the great height to which these accumulations rise,—such are the prominent features of the erratic formation. The rocks of which these blocks are composed in Western Switzerland, are almost all found *in situ* in the valley of the Rhone and its lateral valleys. The hardest rocks furnish the largest blocks; M. de Charpentier mentions many blocks of granite from 40,000 to 100,000 cubic feet, and, as an exception to the general rule, a block of limestone, the largest known, situate near Bex, which contains 161,000 cubic feet. These blocks must have travelled a great distance, some of them 25 and even 60 leagues, from the place of their origin.

According to M. de Charpentier, the deposits of erratic blocks appear under three different forms; sometimes *scattered,* in blocks dispersed here and there, or in insulated blocks, and more or less covered with earth or diluvium. This is the most common form. Sometimes they are *accumulated,* or collected in mounds or small hills, presenting the same modifications and the same structure as moraines. Finally, at other times they are *stratified* in short thick beds, and readily distinguished from the diluvial formation by the great number of angular blocks they contain.

*One curious fact* is the assemblage of blocks of the same species of rocks. M. de Charpentier mentions, among others a band of large blocks near Monthey, in the Valais, from 300 to 800 feet broad, and three-fourths of a league long, which is wholly composed of granite with large grains of felspar, derived from the mountains bounding the valley of Ferret, eleven leagues distant. Some of them contain 60,000 cubic feet, and they are well preserved.

We shall not again refer to the singular forms and curiously balanced positions observed in many blocks. With regard to their nature, M. de Charpentier remarks that, if blocks of gneiss and granite seem less common in our days in the plains than on the mountains of Jura, it is partly owing to the fact that, in the former, a great number have been destroyed in order to facilitate cultivation, or to be employed in building, while those, such as conglomerates, which were too hard to be destroyed, have been allowed to remain. In general, the number of blocks is in the inverse ratio of the progress of industry; and, independently of it being probable that the accumulation was really greater on the sides of the Jura than in the plain, it is evident that, in the former position, they have been more secure from the hand of man.

On the sides of Alpine mountains, the erratic deposit rises to 2200 and 2500 feet above the bottom of the valleys. On the Jura it forms a curve, the upper part of which is on the Chasseron, in front of the great valley of the Rhone, at a height of 3100 feet above the plain, or 4250 feet above the sea; the extremities of the curve reach the plain, one on the side of Soleure, the other near Gex. This formation is distributed in an irregular manner throughout all the valleys of the basin of the Rhone, also on the sides of the mountains, and over all the plain, from Soleure as far as Mont de Sion, at the western extremity of Switzerland.

M. de Charpentier then passes in review the various hypotheses which have been brought forward by different geologists, for the purpose of explaining the mode of transportation of erratic debris. Some (Dolomieu and Ebel) have supposed that the Alps at first presented a uniform and inclined plain, on which the blocks have rolled or slided as far the Jura. To this M. de Charpentier replies, that according to the distance and height from which these blocks have come, the inclined plain could only have had a slope of 1° 8′ 50″. What, moreover, can have become of the removed formations, and what agents can have transported them to another place?

Others have fancied a kind of rafts formed by the ice of glaciers, and transporting the blocks on the waters of a sea, whose level reached the highest elevation that they have attained. M. de Charpentier replies that, according to the facts observed, the shores of this pretended sea, far from having a horizontal surface, must have sometimes had an inclination from the Upper Valais to Vevey, sometimes a curve with a double descent, from Chasseron on the Jura, to Soleure and Gex. Some geologists have thought that the blocks, enclosed in the masses of ice proceeding from glaciers, have been carried forward by currents of great velocity, or else, having fallen on the ice which covered the great lakes, which are supposed to have existed in successive stages in the valleys of the Alps, they had been transported by the masses of ice resulting from the breaking up of this ice on the return of heat.

De Luc imagined that an eruption of gas had projected the erratic blocks, but this hypothesis, so little in accordance with what has been since observed in the circumstances attending these blocks, can no longer be adopted by any one.

Many geologists have ascribed the transportation of the erratic matters to currents of water. Some, like Saussure and and De Buch, conceive that these currents have been owing to a sudden movement, or a rapid retreat of the ocean; others, like Escher de la Linth, to an instantaneous bursting forth of the vast lakes, which may have existed in the interior valleys of the Alps; and lastly, others, such as M. Elie de Beaumont, to the sudden melting of the ancient glaciers, a melting produced, at the period of the rising of the principal chain of the Alps, by the action of the gases, to which he attributes the origin of the dolomites and gypsum. The latter remarks that the Alps, having been formed by many different acts of elevation, must have had snow and glaciers accumulated upon them at the time of the last catastrophe which gave them their present relief, while the Pyrenees, raised by a single movement, do not, according to him, present us with any erratic formation. On this point, M. de Charpentier, who has carefully examined the Pyrenees, affirms that erratic blocks are found there, in a great number of places, and in the same circumstances as among the Alps; and he assures us, that if he has not given a special description of them in his *Essay on the Geognostical Constitution of the Pyrenees*, the fact only proves the ignorance in which he then was respecting the cause of the transportation of these debris. Independently of the objections which M. de Charpentier advances against each of the causes alleged as producing these supposed great currents, he thinks that the latter, whatsoever may have been their origin, cannot explain the dispersion of erratic blocks. He refutes by facts the support which this hypothesis was thought to receive from the effects of the débâcle of Bagnes. The want of selection, according to the size of the blocks, so remarkable in the erratic formation, where the largest are often carried furthest,—the absence of all traces of the shocks which debris so considerable, projected with a velocity estimated at from 175 to 354 feet in a second, should have produced on the side of the Jura facing the valley of the Rhone,—the impossibility of supposing water so charged with debris and mud, as to support the blocks at the surface, since these materials are no longer found, or to understand how they should not have fallen after issuing from the enclosure of the valleys; the state of preservation of a great number of them; the form and situation of the accumulated deposits; the groups of the same species of rocks; the fantastically balanced position of many blocks;—all these circumstances furnish M. de Charpentier with sufficient arguments, in his opinion, to render the idea of the transportation of erratic blocks by currents of water inadmissible.

He at last comes to the hypothesis brought forward by M. Schimper in a German ode (die Eiszeit), and developed by M. Agassiz, of an inclined plane of ice on which the erratic debris have moved along from the Alps to the Jura. This supposition was explained in detail in the Bibliothèque Univ. de Gèneve (Feb. 1841), to which we have already alluded. M. de Charpentier cannot admit it. He observes that the author has not signalized any of the causes which could have produced the excessive sinking of temperature, which he supposes to have taken place before the final elevation of the Alps, any more than those which have induced a milder climate. He is of opinion, that if it be necessary to admit a cooling, which he is ready to do, although to a much less degree than M. Agassiz, this cooling must have taken place after the catastrophe which gave the Alps their present form, and that it has even been the consequence of it. He cannot comprehend how a single winter could have been sufficient to accumulate ice to the height of 3100 feet above the plain; and consequently the excessive cold necessary to freeze the lakes, such as that of Geneva, which does not freeze at—25°C., (–13°F.) must have continued without mitigation for many years. But then this cold must have dried up all the running water, rain would naturally cease, and even snow, which is very rare in very intense colds. But even if we were to admit a thick bed of snow, it would have been too soft and rough to allow the blocks to slide on it to any considerable distance.

Again, when M. Agassiz supposes that, at the moment of the elevation of the high Alps, the sheet of ice, pierced and raised upwards, became a bed for the blocks, M. de Charpentier remarks that, by calculating the mean height of the Alps at 11,000 feet, the greatest elevation of the blocks at 4250 feet above the sea, and the mean distance of the highest parts of the Alps from the Jura at 25 leagues, we find that the supposed surface of ice would only have an inclination of 1° 8′ 50″. Now this slope is too small to admit of blocks with a rough surface and sharp angles sliding upon it such a distance. In fact, according to M. de Charpentier, we never see blocks sliding downwards even on the steepest glaciers, even although the surface is free from snow and hardened by the cold, as sometimes happens in the end of autumn. M. de Charpentier then asks why the limits of the erratic deposit describe on the sides of the Jura a curve and not a horizontal line, such as one would expect from a frozen lake? Why the greatest accumulation of these blocks is found at the highest part of this curve or in its vicinity, the very place where the inclination must have been least? And, lastly, why this accumulation is precisely in front of the great valley of the Rhone, which did not exist before the elevation of the Alps, and could have no influence on the form of the sheet of ice? M. de Charpentier is likewise unable to understand how we are to explain the marks of friction on the rocks by a movement of the sheet of ice analogous to that observed in glaciers. In truth this movement in the latter is owing to the congelation of absorbed water; but the congelation of lake or rain-water can only produce ordinary ice, which is neither capable of absorbing water, nor of moving, nor of polishing rocks. The ice of the lakes of the Pyrenees which never thaw, and that of the frozen marshes in the north of Siberia and America, have never assumed the form of glaciers, which appear to exist only by the névé being gradually converted into ice.

It is from the whole of these considerations taken together, although we can present them only in an abridged form, that M. de Charpentier conceives himself warranted to conclude, that the hypothesis of an extended sheet of ice, is inadequate to explain, in a satisfactory manner, the transportation of the erratic substances of the Alps.

After having thus passed in review all the theoretical suppositions hitherto made on the mode of the dispersion of erratic blocks, the author comes to that which appears to him the most probable, and to which his observations have contributed to give great weight. It is that which ascribes the fact of this dispersion to the action of glaciers, which, if it were once universally adopted, would enable us to define the erratic deposit as a *detritical formation deposited by glaciers*, while the diluvial would be a *detritical formation deposited by water.*

A rather curious remark of M. de Charpentier's is, that he is not the author of this hypothesis, nor even M. Venets, who was the first, however, that supported the suggestion by direct observations. It appears that before this time, in 1815, Playfair, in the notes published on his journey among the Alps, regarded the glaciers as the only agent capable of transporting enormous blocks to great distances without destroying the sharpness of their angles, and that he was not afraid of the extent which, in that case, it was necessary to ascribe to glaciers. The celebrated Goethe also, in the last edition of his *Wilhelm Meister*, published in 1829, advanced the same opinion. Lastly, M. de Charpentier mentions the singular fact, that a belief in the transportation of erratic blocks by glaciers, greatly superior in size and extent to those now existing, is shared by many of the simple mountaineers of the Alps, who, whether from vague tradition, or, what is more probable, the habit of observation which is peculiar to them, have anticipated geologists by admitting this theory as a fact. It likewise appears that M. Esmark of Christiania had admitted,* in 1827, that the blocks of granite dispersed in such great numbers through Norway, had been conveyed by ancient glaciers.

But, confining for the present the application of the theory to the Alps, it must be admitted that, after the last elevation of this chain of mountains, the warm climate (about 17°.5 (63°.5 F.) which had, till then, prevailed in their vicinity, and which was sufficient to allow palms to flourish, since remains of them are found in the deposits formed at their base, gave place to a cold and humid climate; that, during this epoch, glaciers were formed on the highest summits of the Alps and on the most elevated ridges of the secondary chains; that these glaciers increased to such an extent that they descended to the lateral valleys, and filled them to a certain height, and finally reached the great principal valley, where they united into one which ended by debouching into the basin of lower Switzerland. Thus all the great valleys of the Alps would furnish an extensive glacier reaching to the plain situate at their foot; but one only, that of the valley of the Rhone, would acquire such extension as to traverse the plain and reach almost to the highest points of the Jura. The return of heat would gradually melt these enormous glaciers, and reduce them to their present dimensions, while the debris which they carried with them, as is seen in modern glaciers in our own day, would serve as marks to point out their progress, and constitute the erratic formation.

Of the hypothesis of great diluvial glaciers thus announced, M. de Charpentier endeavours to demonstrate that it explains all the phenomena observed respecting the distribution of the erratic formation, when we take into account the facts presented by existing glaciers. Thus the erratic matter of each of the great valleys of Switzerland always presents a collection of all the rocks entering into the composition of the mountains of that valley and of those which open into it. It is the same with the moraines and beds of glaciers, which contain all the rocks belonging to the mountains which encompass them.

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* See Esmark's paper on the Geological History of the Earth. Jameson's *Journal, vol. ii. p. 113.*

The form of the fragments, and the state of preservation of the blocks, are the same in the one case as in the other. The well preserved blocks are generally the largest, because rolling to a greater distance at the moment of their fall, they reached the back of the glacier and remained there during the whole time of its progress.

There are no blocks of too large a size for the expansive power of the ice, and M. de Charpentier mentions a block of serpentine in the valley of Saas of 244,000 cubic feet, which has been transported for about a hundred years, by the present glacier of Matmarck.

The three forms of deposit in the erratic formation, scattered, accumulated, and stratified, answer to the debris of the beds of glaciers, to moraines, and to the *alluvium glaciare.*

The want of selection in the blocks and groups of the same species of rocks, the singularly balanced position which a certain number of them present, are explained in the most satisfactory manner by the hypothesis of glaciers, those of our own day exhibiting the same phenomena.

The comparatively larger quantity of blocks disposed on the flanks of the Jura than in the plain, is explained by this, that lower Switzerland, which served as a bed to the glacier, had no moraines formed in it; the debris which it supported became scattered at the moment of its destruction, and the free lateral expansion to the right or left did not admit of accumulations of debris on the flanks of the glacier. But it was not thus with the flank of the Jura, upon which the great glacier of the Rhone rested; there the blocks accumulated and formed a *great frontal moraine* which is disposed on the declivity of the chain of mountains which formed a limit to the glacier.

The most elevated debris of the erratic formation are the lateral moraines deposited at the moment when the diluvial glaciers had acquired their greatest extent and thickness. The elevation of these debris indicates this maximum. Thus, in the upper Valais, from Aernen as far as Brigue, the glacier must have been 2800 feet thick. At Brigue the valley enlarges, the glacier must necessarily have become lower, and, in fact, the debris rise only to 2500 feet above the Rhone. This height has continued for an extent of 17 leagues, as far as Martigny, the width of the valley remaining nearly the same. It becomes narrower from Martigny to St Maurice, and the debris rise to nearly 3000 feet, then fall to 2300 feet till we come to the lake of Geneva, the glacier having necessarily become lower owing to the great enlargement of the valley of the Rhone. Having reached the basin of the lake, the glacier was freely extended to the west and in the direction of Thonon, its lateral edge sinking to the level of the lake. To the east, on the contrary, the plateau of Jorat forced it to rise to 2600 feet, an elevation indicated by the debris deposited on the mountain of Playau. After passing the Jorat, the diluvial glacier of the Rhone arrived at the Jura. There the insurmountable obstacle presented by that chain of mountains put a stop to its progressive movement; the glacier rose upwards, and deposited its frontal moraine at 3000 feet above the lake of Neuchâtel; while on the two sides, encountering no obstacle, it became enlarged by diminishing the thickness, and described, on the sides of the Jura, the curve now presented by the debris of the erratic formation, which terminate on the one side near Soleure, and on the other near Gex.

The manner in which the erratic formation terminates, sometimes in the form of mounds or bands analogous to moraines, sometimes in scattered debris, or by mingling gradually with the diluvium, which must have been necessarily conveyed by the action of the mighty torrents which escaped from the sides of the diluvial glaciers; at other times, finally, becoming confounded in a way which cannot be disputed, with the debris and moraines of existing glaciers,—are all facts which seem to plead powerfully in favour of the hypothesis of glaciers.

With regard to the considerable extent which the erratic formation of the valley of the Rhone seems to present, it is easy to understand why the glacier furnished by this valley should have been so infinitely larger than any of those which have reached the plain at the bottom of the Alps. In fact the Valais, for four-fifths of its length, is bounded by two of the highest chains of the Alps, and receives the greatest number of lateral valleys proceeding from mountains of sufficient elevation to be still covered with glaciers. Thus the valley of the Rhone receives 32 of these lateral valleys :

| | | |
|---|---|---|
| The valley of the | Rhine . . . | 18 |
| ... | Reuss, . . . . | 11 |
| ... | Arve, . . . . | 8 |
| ... | Aar, . . . . | 7 |
| ... | Limmat, . . . | 7 |
| ... | Sarine, . . . . | 2 |

and it is found, in point of fact, that the erratic formation of the valley of the Rhone is most extended, and that of the Sarine least so.

Lastly, the effect produced even in the present day by glaciers on the rocks which form their beds, renders it easy to account for the marks of wear and friction presented by the rocks in the neighbourhood of erratic debris. But we must beware of supposing that all polished rocks have been rendered so by glaciers ; and M. de Charpentier cannot, for example, admit that the famous polished rock of the Col de la Fenêtre, near the Great St Bernard, is the result of the friction of a glacier, as M. Agassiz seems to think. He ascribes it, as was done by Saussure, and more recently by M. Leonhard, to the friction of the masses against each other, and to a kind of vitrification consequent upon that friction.

Now it will be asked of M. de Charpentier, how he can explain the existence of a climate fit to give the diluvial glaciers of the Alps the gigantic development of that of the Rhone, for example, which was of such breadth as to cover all that part of Switzerland between Soleure and Geneva, and must have been sixty leagues long. He is of opinion that, although it may be impossible for him to reply and assign a probable cause for this great climateric change, we ought not on that account to reject his hypothesis, if in other respects it affords a good explanation of all the facts, just as we have not disclaimed the theory of soulévements, although we are ignorant of their cause. But he thinks he can account for the long series of cold and rainy seasons necessary to the development of diluvial glaciers, from the very consequences of the last elevation of the Alps which immediately preceded it. He entirely renounces the opinion he had expressed in 1834, respecting a probable elevation of the Alps very much above their present level, and he ascribes the change of climate to the numerous crevices and fissures which the upward projection of the high Alps must have produced in the strata overturned by this formidable cataclysm. All these crevices, the largest of which, filled in their lower part, constitute at the present day the valleys of mountains, must have served as passages for the waters, which were thus enabled to penetrate to a great depth into the bosom of the earth. Soon reduced to vapour by the heat of the beds over which they ran, they became condensed in the atmosphere in the form of rain or snow, and the temperature of the walls of the crevices must in this way have rapidly diminished. M. de Charpentier cites on this subject, on the authority of M. Poeppig, the case of the volcano of Anduco, in Chili, from which escape white vapours, which are neither warm nor fetid, but very humid, and which are seen to change into clouds under the eyes of the observer. Thus, over a great extent of the globe, a great quantity of vapours must have been disengaged for a long period, and these, augmenting the humidity of the atmosphere, and becoming transformed into fogs and clouds, intercepted the rays of the sun, and in this way contributed to diminish the temperature. In this way he establishes a long series of rainy and cold seasons, singularly favourable to the formation and development of glaciers, which established themselves wherever the mountains were high enough to permit, and particularly among the Alps. And it is not necessary for this purpose to suppose an excessive degree of cold. That of the years which succeeded each other from 1812 to 1818 would be fully sufficient, according to M. de Charpentier, supposing that it was continued for a long enough time, to explain all the development hypothetically attributed to the diluvial glaciers of the Alps. A very great cold would be even contrary to the theory of glaciers, because rain or liquid water is necessary for their formation and increase. It is probable that this change of climate was the cause of the gradual extinction of beings which had lived up to that time; they were enclosed among the ice formed at this epoch in the north of the old continent. The diluvial glaciers of the north, favoured by climate, acquired even greater extension than those of the Alps, and dispersed to still greater distances the debris they carried along with them. Lastly, in proportion as the crevices and fissures created by the dislocation and fracture of the beds became closed up, and the water ceased to penetrate in such great abundance into the interior of the globe, the vapours diminished, and the hygrometrical and meteorological state of the atmosphere underwent modification. Rain became less frequent, the rays of the sun became more active, and the diluvial glaciers, at once deprived of water and exposed to a higher temperature, must have melted by degrees and returned to their present dimensions.

As to the considerable time necessary for the formation of these immense diluvial glaciers, M. de Charpentier first remarks that authors are not much in the habit of hesitating about time in their geological hypotheses. No one refuses to admit very long periods of years, when attempting to explain the formation of thick beds of sandstone or limestone. But his hypothesis does not require much in this respect. In fact, in 1818, the glacier of the Rhone advanced about 150 feet; by supposing a similar annual progression, it would require 774 years to extend to Soleure, that is to say, 66 leagues from the bottom of the Valais. Thus, a climate analogous to that which prevailed in Switzerland from 1812 to 1818, continued for about eight centuries, would be sufficient to enable the great glacier of the Rhone, augmented by all the glaciers proceeding from the lateral valleys, again to advance and deposit its moraines on the sides of the Jura, where we now find the debris of the erratic formation. In 1818, the alarm was such among the mountaineers in the valley of Chamouni, in consequence of the considerable enlargement of the glaciers, that no one doubted that if the cold and rainy weather of the six preceding years had continued for five more, all the glaciers would have become united, forming only a single glacier, which might have extended as far as Sallanches, situated 7 leagues from thence.

With regard to the crossing the Lake of Geneva, or other lakes filling up the openings of the great valleys, such an obstacle was inadequate to stop the progress of glaciers. When once the water of the lake cools to zero by the melting of the ice, the water will support the glacier, if it is so deep that it cannot reach the bottom. In fact the density of ordinary ice is about 0.92, and that of the porous ice of glaciers must be still less considerable. Thus, the glacier of Panérossaz, in the Alps of Bex, is in great part supported by a lake; in like manner, in 1817, the glacier of Schwartzberg crossed the whole breadth of the lake of Matmarck; and we also know, as Scoresby observed on the coast of Greenland, and quite recently as Captain Ross found near the islands he discovered in the neighbourhood of the south pole, that glaciers advance into the sea to the distance of many miles.

We pass over without remark other objections of detail to which M. de Charpentier endeavours to give answers, either by taking it for granted that they will be urged against him, or, because they have really been started since his hypothesis of diluvial glaciers has been made known to the scientific world; and we terminate this short analysis of his work, by a few words on the influence which he ascribes to glaciers on the diluvial phenomena. The three principal of these phenomena are: the configuration of the surface of the valleys, and of the plain between the Alps and the Jura,—the deposit of diluvial matter,—and the transport and dispersion of erratic blocks. These three phenomena, which are continued to our own day, although on a very reduced scale, have been in some degree contemporaneous in the diluvial epoch; but the actions which predominated, succeeded each other in the order in which they are announced. In fact, if, as M. Agassiz thinks, the dispersion of the erratic debris had taken place first, and at the moment when the Alps pierced the sheet of ice, which he supposes to have then existed, we ought to find these debris generally covered and buried up by the diluvium, which has not been observed to be the case.

The partial filling up of crevices, whether by means of large fragments of broken rocks which still project from the bottom of the valleys, or by the substances carried down by the waters penetrating by their fissures, must have commenced immediately after the rising of the mountains. The torrents formed before the commencement of the era of glaciers, must have carried along much debris, which, after falling into, and levelling the bottom of the valleys, discharged into the lakes, that close up their entrance, all the materials not previously deposited. The basins of these sheets of water were thus sensibly contracted.

The second phase was that of the formation of glaciers and the transport of erratic blocks. This dispersion took place at first in the highest valleys, then gradually in the low regions of the Alps. When the glaciers had passed over the lakes, the blocks of alpine rocks they transported arrived in lower Switzerland, and there formed moraines, bands, and glacial deposits; those which remained on the back of the glaciers produced, on the melting of the latter, scattered deposits; lastly, such as were carried along by torrents, formed new beds of diluvium. These torrents produced by the melting of the glaciers could not fail to be very considerable, and they must have modified the relief of the plain, by mingling with the debris already deposited, such as had been brought down by the glaciers of the high Alps. They must have formed two great rivers, one flowing into the basin of the Rhine, the other, into that of the Rhone. At the moment of the general melting of glaciers, they must have been such as to carry blocks of considerable size to great distances; and it was perhaps at this period that the blocks of alpine rocks, 3 feet in diameter, observed by M. Elie de Beaumont, in the neighbourhood of Lyons, were transported. Moraines could not be formed in the portions of the sides of a glacier which formed a passage to these rivers, for as soon as the blocks were detached, they were carried away to some distance by the water; and it is this that explains why it happens between Gex and Geneva, as between Soleure and Herzogenbuchsée, that at the two lowest points which the diluvial glacier of the Rhone reaches, the erratic formation, instead of terminating in a moraine, passes into the state of diluvium.

The melting of the diluvial glaciers forms the last phase of the diluvial period. The blocks which were up to this time disposed only along the sides of glaciers, were now scattered here and there on the same bed they had occupied. These debris continued in their place wherever water had not access to them; and where torrents were formed, only the large blocks remained. The small debris carried farther, formed new beds of diluvium, which have often interred the large erratic blocks found in them. When the melting of the ice had confined the glaciers within the lakes, their influence on the formation of Lower Switzerland would entirely cease; and the materials which the torrents, to which they give rise, continued to carry along with them, as they do to the present day, were arrested by the lakes, and accumulated at their upper ends. Once more restricted to the valleys of the Alps, the glaciers experienced alternations of progression and retrocession analogous to those we witness in our own times, and which explain the various stages of moraines to be seen on the flanks of mountains on the two sides of the valley. But when the vapours diminished greatly, and the atmosphere cleared, the rains becoming less frequent and the sun warmer, the glaciers melted more and more, and retired within the high valleys, where we find them in the present day. This influence of glaciers, formerly so considerable, on the configuration of the surface of the valleys and basins of Switzerland, is still continued though on a very small scale. They still transport debris, form moraines, leave scattered deposits; in a word, create an erratic formation around them. Torrents carry away a part of these materials, heap them up in the lakes, and form diluvium; so that the present geological epoch is nothing else than a feeble continuation of that which preceded it.

Such are M. de Charpentier's views on the formation of the erratic deposit; views founded on a long series of studies and observations, and which he brings forward with a noble and modest simplicity, and with that entire absence of all irritation against the objections that may be made to them, which characterise, in such a high degree, this distinguished philosopher. His theory, it will be perceived, differs from that of M. Agassiz, inasmuch as the latter, although admitting that the glaciers of the Rhone may have extended as far as the eastern banks of the lake, to Vevey or even to Lausanne, yet cannot conceive that they could reach to the Jura; and is therefore of opinion, that the blocks met with on the sides of this chain of mountains have been brought thither by means of a great sheet of ice, forming an inclined plane from the summit of the Alps. Ice, it is true, is thus the principal agent in both hypotheses; but the one supposes a very considerable change in the temperature, and a state of things which nothing of our own times can enable us to form an idea of; while the other is founded on the direct observation of actual facts, and requires nothing more from the imagination of the reader than to prolong, for a sufficient time, the exceptional circumstances, of which we yet witness occasional instances. This theory of diluvial glaciers has already made much progress among geologists, and its application is by no means limited to the erratic deposits of the Alps. At a great number of places, in England, Scotland, and Norway, some have thought they recognised facts analogous to those on which M. de Charpentier has founded his hypothesis; and now that the impulse is given, it is perhaps more to be feared that we shall see a good many geological phenomena ascribed to the influence of glaciers, which the author of the theory would himself have excluded, than that doubts will be thrown upon the facts which he has so well described and so conscientiously observed.[*]

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## *On the Glacial Theory.* By Roderick Impey Murchison, Esq., President of the Geological Society, &c.[†]

From a study of the Alps, where Venetz and Charpentier led the way in shewing that a connection existed between the erratic blocks and the advance of glaciers, Professor Agassiz has deduced a glacial theory, and has endeavoured to generalize and apply it even to our own countries, in which effort he has been supported by my predecessor in the Chair. In the following observations, I will endeavour to point out what new materials have been brought forward, abroad and at home, to enable us to reason correctly on this difficult question, and I will then suggest some essential modifications of the new hypothesis.

As propounded by Agassiz, the glacial theory, even in its application to the Alps, has met with an opponent in the person of Professor Necker de Saussure. In the first volume of a work which he is now publishing, M. Necker treats, in great detail, the whole subject of superficial detritus connected with the northern and western watershed of the Alps, and gives us the fruits of many years of observation. Adding very considerably to the list of phenomena of transported materials collected by M. A. de Luc, he takes his own illustrious ancestor, De Saussure, as his model, and following in the track of the historian of the Alps, he endeavours to enlarge and improve upon that great observer's suggestions. Pointing out the distinctions between two classes of detritus, viz. one of high antiquity and another of modern date, M. Necker contends that the enormous masses of the ancient drift or diluvial detritus have a direct connection with the actual configuration of the surface, because the *chief* part of them has been derived from the centre of the chain, the flanking and lower mountains, and even the strata on which it rests, having contributed comparatively little to the great advancing body. Examining the high valleys about Chamouni and the foot of Mont Blanc, and finding massive walls from 300 to near 600 feet in height, composed of this ancient diluvium in its coarsest form, near the extremities of certain glaciers, he concludes that they were once the moraines of glaciers which melted away and retired from them. He then goes on to suppose that when the recession of the glaciers took place (an effect which he refers to the same cause as De Saussure), such transversal moraines formed dykes standing out at some distance from the mountain and barred up lakes formed by the melting of the snow and ice. These lakes, at length swollen to excess, are supposed to have burst through the moraine barrier, and to have drifted the materials of which it was composed into the lower countries. M. Necker believes that when these ancient glaciers existed, the Alps were considerably higher than at present, and he judges that such was the case, because the "aiguilles" of Mont Blanc have been lowered very considerably in our own times. Arguing that great blocks are never found at the foot of mountain chains which have not permanent glaciers, of what De Saussure called the "first class," he cites many negative examples, and brings forward the Pyrenees, where no true erratic blocks are seen, as a proof that the minor or second class glaciers, which there occur, never advanced sufficiently far to dam up water-courses, and thus to form those great lakes, to the letting off of which and to the destruction of vast moraines, he attributes the presence of large boulders in the Alps.

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[*] From Biblioth. Universelle de Gèneve, No. 74, p. 390.

[†] From the address delivered at the Anniversary Meeting of the Geological So [illegible] don, 1842.

I must, however, remind M. Necker, that if he assumes that all great erratic blocks are to be referred to some *neighbouring* chain, now the seat of glaciers, he forgets the cases in Scotland and England, and indeed many others, far removed from mountain ranges, and which must be classed, as I shall presently shew, with submarine deposits. Indeed, by far the widest spread of erratic blocks with which we are acquainted, extending over the plains of Germany and Russia, must have taken place (as I believe at least) when those flat regions were beneath the sea; for recent observations have shewn, that the blocks constitute the uppermost or last surface deposit in tracts which exhibit, here and there, proofs of having been an ancient bottom of a sea. But without extending his theory to other parts of the world, it does not appear to me, even when confined to the Alps, that M. Necker explains satisfactorily how the granite blocks of Mont Blanc should lie upon the Jura, by any reference to subaërial debacle; for if we are to imagine the deep hollow of the lake of Geneva filled up with gravel, sand, and mud, and forming an inclined talus from the centre to the flanks of the chain, the subsequent scooping out of this enormous mass of materials involves an intensity of degradation as difficult to believe in as the former extreme climate of Agassiz, by which thousands of feet of snow and ice are supposed to have occupied the same deep valley. I ought not to omit to state, that one of the chief elements introduced by Agassiz into this question, the polished and striated surfaces of the rocks, has not yet been alluded to by this author, but will be treated of in his second volume.

In the mean time, however he may fail to account satisfactorily for the transport of the very distant great blocks, we have to thank M. Necker for the additional materials, which seem to establish one fundamental fact in reference to the Alpine case, viz., when this detritus was cast off, the gorges and flanks of the chain had nearly the same reference to the central crest as that which now prevails. If this be proved, the theory which depends chiefly upon the supposition, that a great elevation of the centre of the chain broke off the ice and dislodged the glaciers, is deprived of its chief basis. In what manner Professor Agassiz can account for the Alps being a great centre of dispersion *when at a lower level,* is indeed a part of his theory which is not easily comprehended. On the other hand, whatever we may think of M. Necker's hypothesis, it must be admitted that the facts adduced by him support one essential point of the glacialists, by connecting the presence of blocks with the existence of glaciers in the Alps, the former being, as he states, invariably found both in the southern and northern watersheds of those mountains, and at the mouths of the great transverse ravines which lead up to the regions of perpetual snow, and in all such cases he allows that the condition of the blocks is highly indicative of their having once formed part of the "moraines" produced by former glaciers.

But the important point, that the glacier is the chief source of the origin of erratic blocks, is entirely denied by another antagonist to the theory of Agassiz, who has appeared in the person of M. Godeffroy.*

After the observations of two summers in the Alps, this author has become convinced that the materials of the so-called moraines have not been derived simply by the glacier from the solid rock in the higher mountains, but are the re-arranged portions only of a great pre-existing diluvial deposit, which had been accumulated in the radiating valleys during a period of great disturbance, anterior to the existence of glaciers in that latitude. Describing (like M. Necker) one of these "trainées" as having a continuous length of fifteen leagues, he infers that such a mass could never have been deposited by a glacier proceeding from mountains of no greater altitude than the Alps. Arguing that glaciers are merely the condensed or central portions of vast accumulations of snow, forced downwards into the gorges by increasing volume from above, the chief novelty of M. Godeffroy's work is contained in the opinion, that in advancing, these bodies of ice cut through the ancient diluvium or drift, just as a plough-share cleaves the soil ("presso tellus consurgit aratro" being his motto), and threw up some portions into lateral moraines, as well as pressed before them others to form terminal moraines. To the crystalline and mechanical changes which the snow has undergone in its passage into solid ice, is attributed much of the confusion and irregularity of outline so visible in the "aiguilles" and other icy masses of the Alps; and to the same disturbing action is referred the rounded and worn exterior of the boulders in moraines, as contrasted with comparatively angular blocks of the pre-existing drift which have not been in contact with the glacier. I refer you to the work of M. Godeffroy for the explanation of the manner in which he supposes the surface of the advancing or retreating glacier was subjected to lateral overflows or "écroulemens" of stones, gravel, and earth, and also for his theory of medial moraines; but I now bring to your notice his ingenious effort to solve one of the very difficult climatological problems in the Alps. Having shewn how the lower valleys must, from year to year, become more and more encumbered with detritus, he seizes this fact to explain by it alone, both the well-known retreat of the glaciers and the fact brought forward by Venetz and other observers; viz. that roads which existed in certain former passes of the high Alps are now quite choked up with snow and ice—a fact which has been supposed to indicate a sensible decrease of temperature within the historic æra. M. Godeffroy contends, that in ancient times, when the gorges were more open, and the heaps of detritus at the entrance into the lower valleys were less in size and fewer in number, and when consequently the glaciers easily extended to greater distances, the continual and unrestricted supply of snow and ice from many affluents more than countervailed the loss through atmospheric action; but that as the obstacles increased at some distance above the terminal moraine, the lower ends of the glaciers not being so fed as to regain in one season the melting losses of the previous year, the inevitable result was a successive shrinkage and retrocession of the mass. The increase of snow and ice in the upper passes, and the blocking up of the roads, are explained by the same agency; for as soon as the descent of the glacier from the higher to the lower Alps was impeded, it would follow, that the frozen matter of the higher regions, deprived of its previous exit, must find its way into the adjacent upper depressions, and there form those *mers de glace* which have obstructed the road-ways or passes of our ancestors. Thus is the supposed anomaly explained without recurring to any change of climate.*

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* Notice sur les Glaciers, les Moraines et les Blocs Erratiques, 1840.

In that part of our own country to which the glacial theory has been applied, Mr Charles Maclaren, already known to you by excellent geological treatises, has recently published a well-condensed small work explaining the views of Agassiz. The phenomena of glaciers and the general doctrines derived from their study being explained, Mr Maclaren proceeds to analyze those cases of transported detritus in the neighbourhood of Edinburgh to which the theory had been supposed to apply.

A year and a half only has elapsed since Professor Agassiz and Dr Buckland seemed to think, that this district was as rich in proofs of the action of glaciers as many other parts of Scotland which they visited, and as I happened to witness the efforts of my predecessor in this Chair to attach Mr Maclaren to his views, I must be permitted to direct your attention to the practical results at which this gentleman has arrived in some prominent cases.

Observing blocks of greenstone on Arthur's Seat, which, from their peculiar structure, must have been transported from Salisbury Crags, a *lower* hill, and separated from the former by an abrupt valley, Mr Maclaren infers, that if the present surface of the land be argued upon (and in all questions of glaciers this is a postulate), neither glacier, nor iceberg, nor current will explain the fact. It is unnecessary that I should here examine this author's hypothesis, by which, in order to solve the local problem, he restores the inclined stratified masses of Salisbury Crags to such an extent as to give them an altitude in ancient times superior to that of Arthur's Seat; for whether we adopt his ingenious view, involving a mighty subsequent denudation, or suppose that in the oscillations of this plutonic tract the former low and high points of land have been relatively depressed and elevated, it is obvious, from the very structure of the rocks, that in both cases a subaqueous, and not a subaërial condition is called for to explain the appearances, and this too, be it recollected, on the summits of the highest hills in the immediate vicinity of the Scottish metropolis, in and around which the action of glaciers has been supposed to be visible at much lower levels!

Among the examples of the scratched and polished surfaces of rocks near Edinburgh, I do not perceive that the glacialists have grappled with certain appearances on which Dr Buckland formerly dwelt with so much pleasure, viz. the grooved or channeled surfaces of the Braid Hills, first pointed out by Sir James Hall, and which the great chemical geologist attributed to a powerful rush of waters. When I visited the low ridge in question with Dr Buckland and other friends,* my conviction was that these grooves, though then attributed by Dr Buckland to glacial action, are due neither to that agency, nor to any rush of waters, but are simply the result of the changes which the mass of the rock underwent, when it passed from its former molten or pasty condition into a solid state. These appearances differ essentially from ordinary glacial scratches or scorings.† They are, in fact, broad undulations or furrows, and instead of trending *from* the higher grounds to the Firth of Forth, as would naturally be the case if they were due to the expansion and descent of glaciers, they rise up to the very *summit* of the low ridge in a direction transverse to its bearing, and with no neighbouring point of ground higher than that on which they occur. On clearing away the thin turf which barely covered the rock, some of these undulations in the surface appeared wide enough to contain the body of a man, and though observing a rude sort of parallelism, their forms were often devious. As their surface was smooth, not much unlike the usual aspect of the so-called "moutonnés" rocks, the glacialists of our party at first seemed to be proving their case, when suddenly a discovery destroyed, at least in my opinion, their theory; for in the adjacent quarries of the same hill, at a much lower level, and upon beds just uncovered by the workmen from beneath much solid stone, other sets of undulations or grooves were detected, so like to those upon the summit of the hill, that a little atmospheric influence alone was required to complete their identity. My belief therefore is, that the undulations were caused by the action which took place when the stone was solidified.

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* I hoped to have been able to quote the opinions of Professor J. Forbes on this *vexata quæstio*, because it is well known that he was a companion of Professor Agassiz in the Alps during the last summer, but this distinguished cultivator of physical science has not yet published his views on the action of glaciers as affecting the surface of the earth, though he has given to the public a very ingenious sketch, descriptive of a peculiar parallel striation in the solid ice of glaciers.—Edinburgh New Philosophical Journal, January 1842.

Phenomena of a similar nature to the Scottish have been since observed in Wales by our late Fellow, Mr Bowman. Captivated by the glacial theory, and having himself endeavoured to shew that it could even be as successfully applied to the south as to the north of Scotland, he examined the highest region of Wales, with the geological structure of which he was previously familiar, half convinced, *a priori*, that he would naturally find in those mountainous tracts some proof in support of the new views which he had adopted. He, however, quitted that country without having been able to observe any evidence whatever in favour of the Alpine theory, though his journey enabled him to detect several examples of striated rocks, which in unskilful hands might have been mistaken for the effects of glacial action; and these he holds up as warning beacons. After stating that there are, in his opinion, no terraces which any follower of Agassiz can construe into "moraines," whether terminal, medial, or lateral, on the flanks of the mountains of Snowdon, the Arenigs, or the Berwyns, he describes three distinct and differently formed sets of parallel markings which he observed in the newly uncovered surfaces of the schistose Silurian rocks, and shews satisfactorily how such appearances, as well as the tops of the joints, might be mistaken by cursory observers for scratches, although they are in fact due to structure.

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* Dr Graham and Mr Maclaren were of the party, in October 1840.

† Plaster casts of these exist in the Geological Society.

Unlike Mr Bowman, Dr Buckland has not confined his views of the action of glaciers to Scotland, but applies them largely to the north of England and to Wales. He has recently endeavoured to satisfy us, that the rocks on the sides of the chief valleys in the latter country which open out from a common centre of elevation are striated, worn, and polished in the direction of the present water-courses, and these he conceives to be evidences of former *glaciers*, which filled up all the valleys radiating from Snowdon to a distance of many miles from a common centre. I confess I see almost insurmountable objections to this view. Apart from other evidence, the very physical geography of this tract is at variance with the construction of such an hypothesis. In the Alps, and indeed in every other part of the world in which they have been observed, the length of glaciers is in ratio to the height of the mountains from which they advance, or, to use the words of Agassiz, from which they *expand.* Now, whilst in the present days, a small glacier hangs to the sides of a mighty giant like Mont Blanc, having the altitude of 15,000 feet, our Welsh hills, having a height only of 4000 feet, had glaciers, by the shewing of Dr Buckland, of a length of many miles. Again, in the same memoir, which fills so large a portion of the principality with glaciers, the author comments upon certain facts already well known to us, viz. the existence upon Moel Tryfane and the adjacent Welsh mountains of sea-shells of existing species, at heights of 1500 and 1700 feet above the sea, where they are associated with mixed detritus of rocks transported from afar, all of which have travelled from the north, the hard chalk and flints of the north of Ireland being included. How are we to reconcile these facts with the theory that the greater part of the country in question was frozen up under the *atmosphere* in some parts of the same modern period? Unable otherwise to explain how marine shells should be found on mountains which are supposed to have been previously and during the same great period occupied by terrestrial glaciers the accumulation of ages, Dr Buckland invokes anew the aid of the old hypothesis of a great *wave.* This wave, rolling from the north, must have dashed over the mountains to a height of near 2000 feet, depositing, as it went, gravel, boulders and fragments, derived from places 200 miles distant, and transporting also marine shells in its passage. But is it not more natural and accordant with all the data upon which our science has been reared, to suppose that when such shells were deposited, the parts of the mountain so affected were permanently beneath the sea, than to call into play the assumption of the passage of so mighty a wave? At one moment the argument used is, that scratchings and polishings of rock r[illegible]ve been done by ice, because in existing nature it has been found that ice can produce such effects; and in the same breath we are told that beds of shells have been placed on a mountain by an agency which is truly supernatural.

In fact the "glacier" theory, as *extended* by its author, in proving too much, may be said to destroy itself. Let it be limited to such effects as are fairly deducible from the Alpine phenomena so clearly described by Agassiz, and we must all admire in it a *vera causa* of exceeding interest; but once pass the bounds of legitimate induction from that *vera causa*, and try to force the many and highly diversified superficial phenomena of the surface of the globe, into direct agreement with evidences of the action of ice under the atmosphere, and you will be driven forward like the ingenious author of the theory, so to apply it to vast tracts of the globe, as in the end to conduct you to the belief, that not only both northern and southern hemispheres, but even *quasi* tropical regions, were shut up during a long period in an icy mantle. Once grant to Agassiz that his deepest valleys of Switzerland, such as the enormous chasm of the lake of Geneva, were formerly filled with solid snow and ice, and I see no stopping-place. From that hypothesis you may proceed to fill the Baltic and Northern Seas, cover Southern England, and half of Germany and Russia with similar icy sheets, on the surfaces of which all the northern boulders might have been shot off But even were such hypotheses granted, without we also build up former mountains of infinitely greater altitude than any which now exist, we have no adequate centres for the construction of enormous glaciers which imagination must create in many regions to account for the phenomena. The very idea which records the existence of these vast former sheets of ice is at variance with all that is most valuable in the works of Charpentier, Venetz, and Agassiz, whose data, as carefully eliminated from Alpine phenomena alone, would naturally teach us never to extend their application when those conditions are absent, viz. the mountain chain, by the very presence of which the phenomena are explained.

But though the Alpine glacial theory be new, the scratches and polished surfaces of rocks are by no means of recent observation. Many Swedish miners, from the days of Tilas and Bergman, failed not to remark how their mountain sides were furrowed, and in our own times, Sefström* of Sweden, and Böhtlingk† of Russia, have not only narrowly traced them over wide regions, but have endeavoured to account for them. The first of these authors remarked, that nearly all the hard rocks of this country had a "worn or weather side," and a highly escarped or "lee side," the former being exposed to the north and the latter to the south; and having further shewn that the detritus had generally been carried from N. to S., he called the worn face the "weather side," and the higher and jagged extremity of such ridges the "lee side." Extending his observations to many hundred places, he divided these scratches into what he calls normal and side furrows, shewing that in the latter there are frequent aberrations from the persistent courses of the former. Although he had been at first disposed to think, from the data in a given country around Falun, that the normal lines were invariably from N. to S., he afterwards discovered that in large tracts of the South of Sweden the direction was from N.W. to S.E., and in others, particularly along the coasts of Norway, from N.E. to S.W.; all these facts being recorded on a map, which is a most valuable document.

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* *See Taylor's* Scientific Memoirs, vol. iii. p. 81.

† *Jameson's Journal*, vol. xxxi. p. 253.

Since Sefström's work was published, M. Böhtlingk, a young Russian naturalist of great promise, but, alas! prematurely carried to the grave, extended his researches to the northern territories of Russia. Observing that the dominant direction of the scratches in parts of the governments of Olonetz and Archangel was from N. to S., and that along the edges of the Bothnian Gulf their course was from W. to E., he passed the summit level of Russian Lapland, and found that there the drift had no longer been transported from N. to S., or from N.W. to S.E., but, on the contrary, from S.E. to N.W.; or, in other words, that the blocks of Lapland had been carried northwards into the shores of the Polar Sea. In a recent letter to Mr Lyell, read before this Society, Professor Nordenskiöld has accurately recorded the phenomena of this class observed by him in Finland, and he shews that there the blocks and striæ proceed from N.N.W. to S.S.E.

The theory of Sefström and his followers is, that a great flood, transporting gravel, sand, and boulders, was impelled from the north over preexisting land, and that the deviations from the N. and S. direction are due only to various promontories by which the flood was deflected. So convinced was this author that with local aberrations all the transport throughout the whole of Europe had taken place from north to south, that he not only travelled over the whole of Germany, and saw nothing except materials streaming in the same direction, but even carried with him his northern drift into the Austrian and Bavarian Alps. I will not waste your time by pointing out the errors into which his hypothesis, though founded on data good within a limited radius, led this author. Every one who has studied the Alps (and the facts were well known before the days of glacial theories), is perfectly aware that the detritus on their flanks has been shot off eccentrically from the higher central masses. The observations indeed of Böhtlingk give the same result upon a very grand scale in the north, and explain what Sefström, with all his valuable labour, had left unknown, viz. that the Scandinavian mountains, as a whole had produced exactly the same detrital result as the Alps, having poured off their detritus in all directions *from a common centre,* the northern chain differing only from that of central Europe, by the much wider range to which its blocks and boulders were transmitted.

My own belief, Gentlemen, as you know, has been, that by far the greatest quantity of boulders, gravel, and clay distributed over our plains, and occupying the sides of our estuaries and river banks, was accumulated *beneath the waters* of former days. Throughout large tracts of England we can demonstrate this to have been the case by the collocation of marine shells of existing species with far transported materials. It was the association of these testacea with foreign blocks in the central counties of England which first led me to attach a new and substantial value to that view of glacial action which had been so well advocated by Mr Lyell before Professor Agassiz came forward with his great terrestrial and general theory. I am bound to say, that wide researches during the last two years have strongly confirmed my early views.* I could not travel, in the autumn of the year 1840, around the shores of the Highlands of Scotland, without being convinced that the terrace upon terrace, presented on the sides of some of the great valleys, and often high up on the sea-ward hills of the bays opening out to the ocean, were nothing more than the bottoms of former seas and estuaries which had been successively desiccated.

I coincide, therefore, entirely with Mr C. Darwin in his very ingenious explanation of the probable formation of the parallel roads of Glen Roy (Phil. Trans., 1839, p. 39). Since then, that excellent observer has borne out similar views in a paper read before our own Society. In this memoir, estimating the different changes of the sea and land, and shewing to what extent the solid strata were depressed, whose relative histories he thus reads off, he traces the shingle beds from the edge of the sea, where they are in process of formation, to considerable heights inland; and estimating how blocks were transported from the great Cordillera within, or not long before the period of existing sea shells, he explains the far-transported boulders by their being carried to the ports where they lie in vessels of ice. The melting of these icebergs he conceives to have been the chief agent in forming such masses of clay, gravel, and boulders, as constitute the "till" of Scotland; whilst the confusion and contortion of their imperfect strata is considered by him to be necessarily due to the grounding of icebergs in the manner formerly suggested by Mr Lyell. To the same powerfully disturbing agent he attributes the general absence of organic remains in these deposits; and, lastly, he infers that it is much more probable that the great boulders were transported in icebergs detached from glaciers on the coast, than imbedded in masses of ice produced by the freezing of the sea.

M. de Verneuil and myself had previously brought before you some new results, arising from our first expedition to Russia. We endeavoured to shew the utter inapplicability of the Alpine glacial theory to vast regions of Northern Russia, though the surfaces of the rocks are scored and polished, and far-travelled blocks occur throughout a wide area in isolated groups, because much of this detritus has travelled over extensive tracts of low country, from which it has ascended to levels higher than the sources of its origin. Hence we inferred, that the onward persistent march (in many parts up-hill) of a body of glaciers, having a front of many hundred miles in extent, is irreconcileable with any imaginable subaërial action. On the other hand, it was proved, by the presence of sea shells of an arctic character, that the "terra firma" to which some of the blocks had been transported, had been the bed of the Northern or glacial Sea at the period of this transport. We then attempted to explain how the parallel striæ and polishing of the surface of rocks of unequal altitude was reconcileable with the *submarine* action of ice, by supposing that the ice floes and their detritus might be set in motion by the elevation of the Scandinavian continent, and the consequent breaking up of great glaciers on the northern shores of a sea which then covered all the flat regions of Russia; and we further stated our belief, that the bottoms of these icebergs, extending to great depths, must have every here and there stranded upon the highest and most uneven points of the bottom of the sea into which they floated; that where the bottom was hard rock, the lower surface of the iceberg, like the lower surface of a glacier, would grate along and score and polish the subjacent mass; that where the bottom consisted of tenacious mud or clay, the iceberg once fairly stranded would be retained till it melted away, entirely or in part, whilst it would be more frequently borne over sand-banks, on account of their less resistance. In this manner, we endeavoured to explain not only the scratches and polish of hard submarine rocks, but also why large blocks are often found on former submarine hills, and why (in Russia at least) such blocks are more frequently associated with clay than sand. These views were indeed first expressed at the Glasgow meeting of the British Association, when I strove to reduce a large portion of the Alpine glacial theory to considerations depending upon the fact, that during the æra of the dispersion of the large blocks, by far the greater portion of our continents were *beneath the sea.*

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* See Silurian System, p. 536.

Mr Maclaren, to whom I have already adverted, has recently improved this view, by shewing how the parallel scratches and grooves ranging from N.NW. to S.SE., and the dispersion of blocks in that direction, are reconcileable with the union of currents from the N., set in action, as above supposed, by a great polar elevation which acted as a "centre of dispersion;" but, as the author adds, a broad current would also set continually *eastward* along the immersed regions included in the temperate zone; and hence, he says, that when the icebergs were drifting southwards from the poles, they would naturally be carried to the SE. by a stream compounded of the two currents. After reasoning upon the wide application to which the view of floating iceberg action is capable, and how many of our present terrestrial appearances it will explain, Mr Maclaren adds, "Mr Murchison's hypothesis, if adopted, does not exclude that of Agassiz. On the contrary, it may be assumed, that while the glacial condition (which caused the great accumulation of ice in the northern regions) continued, every mountain chain, which *then* had an elevation of 2000 [illegible] 3000 feet above the sea, would be encrusted with ice, perhaps as far south as the latitude of 40°. Each of these would be on a small scale what the polar nucleus was on a great scale, a centre of dispersion."

In the memoir upon Russia by M. de Verneuil and myself, one observation, however, occurs, which has not found its way into the abstracts, and which, therefore, I may advert to, as explaining why the rough detritus of mud, sand, clay, and boulders so very seldom contains marine remains. Such heaps are made up of materials, which we consider to have been imbedded in a true terrestrial glacier, and therefore, though detached, and floated to a distance, they never could afford more than *terrestrial* detritus; and if to this be added the consideration of how the stranding of such masses would destroy animals in the vicinity, as suggested by Darwin, we may rationally conceive why so few shells have been discovered in this coarse detritus, whilst we readily perceive why the stones impacted in it should be scored and striated, and often polished.

Besides the great advancement of our knowledge of terrestrial magnetism, which at some future day may be connected with our labours, the Antarctic expedition, under the distinguished navigator Captain James Ross, has, as might have been expected, thrown considerable light upon the glacial theory. A few years only have passed since the existence of an enormous mass of ice-clad land in the antarctic region, was announced by an American squadron of geographical research. This great icy tract, which was described as exhibiting hills and valleys, and even rocks upon its surface, has entirely disappeared in the short intervening time; for Captain Ross has sailed completely through the parallels of latitude and in the same longitude which it was said to occupy. As we cannot suppose that the American navigators were deceived by atmospheric phenomena, so must we believe that what they took for solid land, was one of the enormous accumulations of ice called "packs," the great source of those enormous ice islands which periodically encumber the Southern Seas.

Continuing his progress towards the South Pole in almost open sea, Captain J. Ross discovered, as he proudly says, "for the honour of England," the southernmost known land, which he named Victoria, and which he coasted for more than 8 degrees of latitude. This land rises in lofty mountain peaks, from 9000 to 12,000 feet in height, perfectly covered with eternal snow, from which glaciers descend, and project many miles into the ocean, terminating in perpendicular lofty cliffs. The rocks which could be examined were of igneous origin, and near the extreme south point of his exploration, or in S. lat. 77° 32′, long. 167° E., a magnificent volcano was seen in full action, emitting flame and smoke at an altitude of 12,400 feet. Further progress to the southward was then impeded by an enormous barrier of ice, or glaciers 150 feet high, which stretched from W.N.W. to E.S.E., and which the bold seaman traced in continuity for 300 miles, to long. E. 191° 23′, and lat. S. 78°. That this barrier was a true glacier was inferred from the existence of a very lofty chain of mountains behind it, the tops of which, as seen from the mast-heads, were estimated to be a degree of latitude to the south of the sea-face of this great wall of ice, at not more than half a mile from which the soundings were at 318 fathoms deep, and upon a bed of blue soft mud. Here, then, the geologist is presented with abundant matter for speculation. Volcanos in the midst of eternal polar snow and glaciers, with seaward faces as wide as some of the continental tracts, which, from the striæ and polish on their surface, and the wide dispersion of blocks and detritus, are supposed to have been affected by former terrestrial glacial action. Whilst, however, we have here the proof that existing glaciers advance some few miles into the sea, we are also informed that the ice ceases suddenly against an ocean 2000 feet deep, and thus we are led to conclude that many glaciers, which may formerly have extended themselves into the sea, had a length, the extent of which, whether like this antarctic example, or those which have been measured in the Alps, was proportioned to the altitude of the ancient mountains against which they rested. By the same reasoning we may infer that the striæ and polish of rocks, or accumulation of coarse detritus, and large blocks which are only to be observed in places far beyond the limits that are now established between mountains and their dependent masses of ice, cannot be due to the advance of former solid glaciers, but must rather be referred, as I have argued, to the floating away of vast packs and icebergs liberated from *centres of congelation.*

But besides the submarine operations now in action, and which may serve to explain most of our ancient phenomena, it has been shewn that in Russia and other cold countries there are several actual subaërial processes, by which large blocks are accumulated at different heights by the expansion of the ice of rivers, or have been piled up by the glacial action of former lakes, when at much higher levels,* leaving lines of coarse angular blocks.

I desist, however, in this place from entering further into the many features under which the existing agency of ice may be viewed apart from the results of the movements of glaciers. More than enough has indeed already been said; for so long as the greater number of practical geologists of Europe are opposed to the wide extension of a terrestrial glacial theory, there can be little risk that such doctrine should take too deep a hold of the mind. But whilst we may have no fear of this sort in Europe, I have lately read with regret certain passages in the Anniversary Discourse of Professor Hitchcock of the United States. In North America, striated, scored, and polished surfaces of rocks, proceeding from N. to S. for vast distances, occupy, it appears, at intervals a breadth of 2000 miles, and are seen on hard rocks at all levels from the sea-shore to heights of 3000 and 4000 feet. Professor Hitchcock tells us, that these phenomena and the accumulations of gravel and blocks had always been inexplicable, until the work of Agassiz unexpectedly threw a flood of light upon his mind.* If Professor Hitchcock could demonstrate what he now seems to believe, that the great mass of the continent of North America was formerly covered with ice, he must first prove that it was not at that period below the level of the sea; but as yet no facts are before us to lead us to doubt that the great accumulation of detritus and the transport of blocks did take place beneath the waters in that country. In justice, however, to this author, it must be said, that in expounding the glacial theory he ingenuously acknowledges the great difficulty of believing that solid masses of ice 3000 to 4000 feet thick, covered the whole region; that no action of a glacier will explain the persistent striation of the surface of an entire *continent* from N. to S. and that the direction of the boulders and the striæ is to a great extent up-hill. When these and many other difficulties shall have been carefully weighed, our transatlantic friends may be disposed to modify their views, particularly when they find that the existence of glaciers in Scotland and England (I mean in the Alpine sense) is not yet, at all events, established to the satisfaction of what I believe to be by far the greater number of British geologists.

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* Geological Proceedings, Murchison and De Verneuil on Russia, vol. iii. *p.* 406.

The presence of Mr Lyell at this time in North America, is indeed most opportune, for whatever changes his mind may have recently undergone, no geologist has more strenuously laboured to make himself master of all its bearings, or more systematically enlarged our knowledge of this disputed subject. Possessing as he now does the advantage of observation on a vast scale, I have little doubt that he will account for the wide dispersion of blocks in America from N. to S., by referring to a cause quite as general and quite as aqueous as that by which he originally sought to explain the phenomena in Europe.†

Although the consideration of this subject has already carried me beyond the limits I had prescribed to myself, yet I cannot quit it without reminding you, that the greatest geological authorities on the Continent, led on by Von Buch who has so long studied these phenomena in his native land, are opponents to the views of Agassiz. Even whilst I write, I find that M. de Beaumont has just communicated to the Institute of France, a report on the results of a journey through Lapland, Finland, and the north of Europe, by his countryman M. Durocher, in which, group-ing the facts with great perspicuity, he handles the whole subject with his usual master's hand, and points out the value of the previous observations of Von Buch, Brongniart, and other writers. M. Durocher conceives that the phenomenon of the transport of erratic matters has proceeded from two successive and distinct operations: the first a great current from the pole, to which the striæ and polish of rocks, and the deposits called Osars, are referred; the second, the transport of the distant blocks by vessels of ice, when all that part of Europe which they cover was subjected to the immersion of an icy sea. He does not agree with M. Böhtlingk, that the point of departure of the current can be placed in Lapland, but supposes it to have proceeded directly athwart those regions from the pole *. But the point to which I now especially advert is, that in his skilful analysis of this memoir our eminent foreign associate admits floating ice as a *vera causa* to explain the drift of blocks, just in the same manner as in common with Lyell, Darwin, and others, I have been endeavouring to explain the phenomenon during the last three years, and thus the inference which was drawn from plain facts is admitted, viz. that the chief tracts covered by erratic blocks were *under the sea* at the period of their dispersion. (Sil. Syst. p. 536.)

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* Anniversary Address. Philadelphia, April 1841, p. 24. I must be excused for stating that Professor Hitchcock has entirely misconceived my view, when he places my name among those who had espoused the Alpine glacial theory. My efforts have been invariably directed towards its limitation, nay, to its entire rejection, as applicable to be by far the largest portions of the surface of the globe.

† See Principles of Geology, 2d edit. vol. i. p. 342; and Elements of Geology, 1st edit. p. 136.

Thus far had I written, Gentlemen,—in short I had, as I thought, exhausted the glacial subject at all events for this year,—when two most important documents were put into my hands. The first of these is the discourse of my predecessor, who has so modified his first views, that I cannot but heartily congratulate the Society on the results at which he has now arrived. I rejoice in the prudence of my friend, who has not permitted the arguments of the able advocate to appear as the sober judgment of so distinguished a President of the Geological Society. In fact, it is now plain that Dr Buckland abandons, to a great extent, the theory of Agassiz, and admits fully the effects of water as well as of ice, to account for many of the long-disputed phenomena. Whilst this admission involves the concession for which we have been contending, viz. that the great surfaces of our continent were *immersed,* and not above the waters, when by far the greater number of the phenomena on the surface of rocks was produced, I reject for those who entertain the same opinions as myself, the simple division into "glacialists" and "diluvialists," into which Dr Buckland has divided the combatants on this question; for to whatever extent the former title has been won by Agassiz and himself, we who have contended for the submarine action of ice in former times, analogous to that which we believe is going on at present, can never be merged with those who, under the name of diluvialists, have contended for the rush of mighty waves and waters over continents. Besides glacialists and diluvialists, my friend must therefore permit me to call for a third class, the designation of which I leave to him, in which some of us desire to be enrolled who have advocated that modified view to which the general opinion is now tending.

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* M. Durocher has made two valuable observations, in shewing us that the striated and polished surface of the hard rocks is sometimes covered by accumulations of sand and detritus; and that although proceeding in a general sense from the north, the furthest transported blocks are so distributed as to indicate *radiation* from certain mineralogical centres, much in the same way as our blocks of Shap granite have, on a less scale, been scattered from one point of distribution. In stating, however, that, in the progress of these transported masses to the south, granitic blocks always constitute the outermost zone, it appears to me that M. Durocher has generalized beyond the field of his own observation. In Russia, for example, M. de Verneuil and myself traced greenstone blocks to the same southerly latitudes as granites. The blocks between Jurievitz and Nijny Novogorod are composed of quartz rock, and of the peculiar trappæan breccia known in Russia as "Solomenskoi-kamen," the parent rocks of which we examined *in situ* near Petrazowodsk (Geol. Proceedings, vol. iii. p. 405), whilst the extreme boundary of these boulders extends to Garbâtof on the Okka, S.W. of Nijny Novogorod, and consequently very far beyond Kostroma, the limit assigned to them by M. Durocher. Again, if M. Durocher prolongs the northern drift to the flanks of the Ural Mountains, he is decidedly in error, for there is no coarse detritus whatever on the flanks of that chain, whether derived from the north or from itself. Of the *Tchornoi-Zem*, or black earth of the central regions of Russia, to which, quoting Baron A. de Meyendorf, M. de Beaumont refers in a long note, I will now only say, that having studied the nature and extent of this singular deposit over very wide regions, I intend, with the help of my fellow-travellers M. de Verneuil and Count Keyserling, to lay before the public very shortly a sketch of its relations to the northern drift and other superficial deposits of

The other point to which I allude, and bearing at once on this view, is a discovery which our Librarian has just made without quitting the apartments which he so truly adorns. In the American Journal of Science for the year 1826, Mr Lonsdale has detected a short, clear, and modest statement, entitled "Remarks on Boulders, by Peter Dobson," which, though little more than one page in length, contains the essence of the modified glacial theory at which we have arrived after so much debate. First describing in a few lines the manner in which large boulders, weighing from ten cwt. to fifteen tons, were dug out in clay and gravel, when making the foundations for his own cotton factory at Vernon, and seeing that it was not uncommon to find them worn, abraded, and scratched on the lower side, "*as if done* (to use his own expression) *by their having been dragged over rocks and gravelly earth in one steady position,*" he adds this most remarkable sentence:—"*I think we cannot account for these appearances, unless we call in the aid of ice as well as water, and that they have been worn by being suspended and carried in ice over rocks and earth under water.*" To shew also that he had read much and thought deeply on this subject, Mr Dobson quotes British authorities to prove, that as ice-floes constantly carry huge masses of stone, and deposit them at great distances from their original situation, so may they explain the transportation of foreign boulders to our continents.

Apologizing, therefore, for having detained you long, and for having previously too much extended a similar mode of reasoning, I take leave of the glacial theory in congratulating American science in having possessed the original author of the best glacial theory, though his name had escaped notice; and in recommending to you the terse argument of Peter Dobson, a previous acquaintance with which might have saved volumes of disputation on both sides of the Atlantic.

In the mean time, however, we may account for the transport of boulders, the striation and polish of rocks, and the accumulation of superficial detritus, we cannot quit the glacial subject without avowing our obligations to Venetz, Charpentier, and Agassiz, and above all to the last, for having brought the agency of ice more directly into consideration as a *vera causa*, to explain many phenomena on the surface. Even we who differ from Agassiz in his generalizations, and have not examined the Alps since the theory was propounded, should not hastily adopt opinions which may be modified after a study of the glaciers *in situ*. "Come and see" is the bold challenge of the Professor of Neuchâtel to all who oppose him, and sanguine as to the correctness of his opinions, he is certain that many will be converted if they would but observe the phenomena on which his views are based. Truly we must acknowledge, that he was the first person who roused our attention to the effects produced by the bottom of an advancing glacier, and if geologists should eventually be led to believe, that certain parallel scratches and striæ on the rocks were in some instances due to glaciers moving *overland*, but in many other cases were produced by icebergs, we must remember that the fertile mind of Agassiz has afforded us the chief means of experimentally solving the problem.

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## *Description and Uses of his Protracting Table.* By GEORGE BUCHANAN, Esq., F.R.S.E., F.R.S.S.A., Civil Engineer, Edinburgh, with a Plate. Communicated by the Royal Scottish Society of Arts.*

This instrument was formerly exhibited in the year 1827, and was then honoured with the approbation of the Society of Arts, and a medal awarded. Since that time it has been in constant use, and I have found it of the greatest service in numerous and extensive surveys which have been laid down by it. Having been lately requested by Mr Buddle, the distinguished mining engineer in Newcastle, to superintend the construction of a similar table for him, this has now been accomplished in a very superior style, having been commenced, and the principal parts formed, by the late Mr Dunn, optician here; the frame-work and other parts of the table by Mr Sandeman, cabinet-maker; and the whole completed by Messrs Adie and Son, opticians, and with a degree of accuracy and perfect workmanship which is highly satisfactory.

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* *Read* before the Royal Scottish Society of Arts, 14th March 1842.

The principle of this machine consists in having the sheet or table on which the survey is to be laid down moveable round a centre, whereby it can be set to any required angle; while the straight edge or ruler for drawing or laying down these angles remains in a fixed position as to the angles, but capable of having a parallel motion across or throughout the sheet. Hence the instrument consists of three essential parts. *1st*, The circular table or drawing-board on which the sheets for laying down the surveys are fixed. *2d*, The protractor or divided circle on which the degrees and minutes are accurately marked; and, *3d*, The parallel ruler and straight edge by which the angles are drawn to any degree or minute, and from any point throughout the sheet on which these angles may have been taken during the survey. The whole apparatus of table, protractor, and parallel ruler, are set on a frame which can be elevated to any angle for the convenience of working. See perspective view.

The board or table consists of a strong mahogany framed circular drawing board, 3 feet 1 inch in diameter, $\frac{7}{8}$ths of an inch thick, calculated to receive a sheet of the common atlas drawing paper 33 inches by 26, which being fixed down along the edges by paste or gum, is stretched out to a smooth and even surface.

The protracting circle extends round the circumference of the drawing board, and the great advantage of this arrangement is, that the degrees and minutes are formed on so large a scale that they can be readily distinguished by the eye, and laid off with the greatest expedition as well as accuracy. In the original instrument, this circle was formed by a circular plate of brass laid along the circumference of the drawing board; but in the present instrument, Mr Dunn has introduced an entire circle or wheel of brass extending from the centre to the circumference, and the intermediate spaces connected by brass of open frame-work. See plan of framing and brass wheel. The exterior circle has been very accurately divided by Mr Adie on his dividing engine into degrees and half degrees, and these again by a nonius into minutes, each of which is most easily distinguishable by the naked eye. The instrument can be set by the hand very nearly to a minute; but for greater accuracy, there is a tangent screw by which this can be accomplished for very nice purposes, and which, by a simple and ingenious arrangement, can be at once attached or detached at pleasure.

The parallel ruler forms a peculiar feature in this instrument. In other methods of protracting, this is shifted about to different parts of the paper, and to different angles, whereby it becomes difficult to preserve the parallelism with accuracy. In the present instrument, the ruler continues invariably on one parallel or meridian, while the various degrees of angular deviation from the meridian, as already stated, are produced by the rotation of the whole board round its centre, and the parallel ruler at the same time ranging with a parallel motion over the whole extent of the board, the angles can in this manner be transferred with readiness to any part of the plan. The ruler being thus fixed in angular position, its parallelism can be preserved with great accuracy, and this is done by means of a roller fixed in the centre of the frame to which the straight edge is attached, with a toothed wheel fixed at right angles to the roller at each extremity; these wheels running in two brass racks fixed in frames at right angles to the roller on each side of the board. These wheels and racks being accurately divided and cut, we obtain a degree of parallelism in the instrument which is almost perfect, and which could not be obtained so simply by any other means that I am aware of. So long as the wheels continue to traverse in the same teeth, there can hardly be an error in the parallelism to the extent of many seconds. Now, the weight of the ruler preserves the wheels in the same teeth; but should any accidental motion or concussion of the instrument drive them out of their true position, the error is soon intimated by a want of correspondence in the different angles which form a check to one another, and then the error can at once be adjusted by a standard index on the frame by which the ruler is brought back to its original position. Should the wheel start upon the rack only one tooth, it would produce an error of only 5 minutes on the angle. The weight of the ruler and wheels is balanced by two counter-weights.

The great advantage of this instrument consists in the facility and accuracy with which the numerous angles that occur in extensive surveys can be laid down. In many cases, I have seen 50 or 60 angles taken from one station in a system of triangulation, all laid down in the course of a few minutes, and the angles from different points meeting and intersecting one another with an exactness truly surprising, and almost rivalling the accuracy of a trigonometrical calculation. This instrument, therefore, must be of great use in laying down surveys generally ; and more particularly, all those surveys now so general which are carried on by a system of triangulation. In all extensive land surveys, I have long since introduced this system, and with great advantage, both as to expedition and accuracy. In county surveys it is indispensable, and also in those surveys for marine engineering and hydrographical purposes, of which a very interesting account has just been given by my esteemed friend Mr D. Stevenson, in his recent work on marine surveying. But above all, I should think the use of this instrument would be invaluable in those extensive trigonometrical surveys of Britain which are now going on under the Board of Ordnance. The great triangles in these surveys extending often to 50, 60, and even 100 miles between the different points, must necessarily be calculated by trigonometry, and still require all the resources and refinements of that perfect science; but for all the subordinate stations, comprehending those whose distances do not exceed 6, 8, or 10 miles, I am satisfied that an instrument of this kind would prove of essential service in filling up the details and expediting the laborious operations of that great work.

GEORGE BUCHANAN.

EDINBURGH, 21*st February* 1842.

# *General View of the Geological Structure of the Alps.* By M. Studer of Berne.* (With a Section.)

*Physiognomy of the Surface.*—An old theory, very generally received, and still much believed at the present time, because it rests on the authority of Ebel's work, represents our Alps as composed of parallel chains, placed one above another, according to their heights, in such a manner that the highest occupies the axis of the system, and, with the exception of some *Cols*, is covered, throughout its whole extent, with perpetual snow. Some of the most frequented passes of the Alps are, it is true, of a nature to favour this view; and the mind, always more or less systematic, easily persuades itself that it is the same water-shed which we pass from south to north, whether we cross the St Gothard, the Brenner, or the *Tauern* of the Tyrol. We only require, however, to cast a glance over a map of Switzerland, in order to convince ourselves that the case is different in reality. In many instances a traveller would be much puzzled if he were asked on what side of the central axis he was. I would instance as an example the Malöja in the upper Engadine, where, from the Maira, which flows into the Po, we arrive in a few minutes at the lake of Sils, whence escapes the Inn, without taking into account the source of a third stream, also near at hand, which runs into the Rhine. Here, therefore, we have the true water-shed of the Mediterranean and the North Sea; and, nevertheless, the mountain mass which supplies these sources has nothing remarkable about it; it has only a few patches of snow on its summit, whereas, to the south, rise the immense peaks of the Bernina, covered with glaciers, and rivalling in height the loftiest elevations of the Bernese chain. A traveller proceeding from the Grimsel to Viesch by the glacier of Oberaar, that is to say, from the sources of the Aar to those of one of the tributaries of the Rhone, and on the following day crossing the Simplon in a comfortable carriage, would not fail to believe that it was during his first day's journey that he had traversed the water-shed, and had passed the central chain. The same traveller would form a very different opinion, if, in order to reach Italy, he had the first day crossed the Gemmi, and the second the Col of St Theodule.

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* This valuable summary of the geology of the Alps is taken from the *Bibliothèque Universelle de Genève*, No. 75, and owes its origin to a lecture given by M. Studer to the party of naturalists assembled last summer at the Hospice of the Grimsel. The essay is founded on the notes taken at the time by M. E. Desor, but was revised by M. Studer, who likewise furnished the explanatory section.—Editor.

A profound study of the system of the Alps has caused M. Studer, and all the geologists who have examined the Alps with care, to abandon the idea of a central chain flanked by parallel secondary chains. According to M. Studer, the Alps are divided into *groups*, forming so many distinct central masses, which run, for the most part, in the same direction, but which frequently, in relation to one another, follow an oblique course, or are arranged like the squares of a chess-board around a central axis, nearly similar to the different crater-cones belonging to the same volcanic zone. The intermediate spaces between the central masses contain particular formations, whose structure and mineralogical nature are related to those of the principal masses; it is in these formations that the most of the interior valleys of the Alps are hollowed, and it is to these that the greater number of the Cols correspond. The secondary formations, such as the limestones, the slates, and the sandstones, stretching along the northern edge, and partly along the southern edge of the high Alps, are intimately connected with the formations proper to the interior valleys; witness, for example, the rocks of the valleys of Frutigen and of Gessenay, which continue without interruption into the Valais, stretching to the east, by the Nuffenen, as far as the southern side of the St Gothard, and to the south, by the Great St Bernard and the neighbouring rocks, into Piedmont. In the same manner also, the limestones and the slates of Coire and of Prettigau, extend to the south over the axis of the Alps, into the Upper Engadine, and as far as Bormio, to unite themselves with the limestones of the Alps of Lombardy, after having traversed the whole Alpine system.

In the present state of our knowledge, it is not possible to determine the limits of all the central masses of the system of the Alps. We are able, however, to recognise six principal masses or groups in the portion which is more immediately in our neighbourhood :—

1. *The group of Mont Blanc*, stretching from the *Col du Bonhomme* to Salion in the Valais, and limited by the valleys of Chamouny and of Entrèves.

2. *The group of the Aiguilles Rouges*, situated more to the north, commencing near Servoz, and terminating near Lavey, above the *Dent de Morcles.*

3. *The group of the Dent-Blanche*, which rises from the bottom of the *Val d'Annivier*, attains its culminating point in the peak called the *Dent Blanche*, and is prolonged obliquely through the middle of the valley of St Nicholas towards the Simplon.

4. *The group of Mont-Rosa*, which lies to the east of the last, and stretches towards the southern portion of the Simplon, and probably also into the Canton Tessin.

5. *The group of St Gothard*, extending from the Upper Valais to the vicinity of Trons in the valley of the *Vorder* Rhine on the north, and bounded on the south by the valley of Airolo.

6. *The group of the Finsteraarhorn*, the largest of all, and that which exercises the most preponderating influence on the *relief* of the surface of Switzerland. The pass of the Gemmi and that of Kisten to the east of the Tödi, may be regarded as its extreme limits. The Col of the Grimsel, from Imgrund to Obergestelen, and the road of the St Gothard from Amsteg to Urseren, traverse it in its whole breadth. The vicinity of the group of St Gothard, and the distance of the other groups, produce the remarkable symmetry of the Swiss Alps to the east and west of the St Gothard.

*Mica-slate, gneiss, granite, &c.*—The groups just enumerated are remarkable, not only on account of the immense glaciers which they feed, and whose study has acquired such interest from the labours of Charpentier and Agassiz, but also from the nature and structure of their rocks. Their principal mass is composed of rocks formerly called primitive, viz. of mica-slate, of gneiss, of protogine, and of gneissitic granite. The direction of the beds, which are generally highly inclined, is conformable to the direction of the central mass. On the two sides, the beds dip towards the central axis, and their inclination becomes more considerable as we ascend the flanks, until they are completely vertical. The arrangement may be compared to a fan, open towards the summit. The rock composing the vertical beds is generally a gneissitic granite,—that is to say, a rock so much approaching to true granite, that we can only see the structure on the great scale, and when we examine it from a distance. In the inclined beds of the flanks, gneiss alternates with other rocks, and we have frequently layers several thousand feet thick. Sometimes also the gneissitic granite makes its appearance there as a subordinate rock. In ascending the valley of Hasli, we see the beds of gneiss and of mica-slate, which form the walls of the valley, dipping to the south as far as above Guttanen, where they give place to a gneissitic granite whose strata gradually become vertical. Near to the Hospice, and at the summit of the Col, the rock has completely the aspect of a true granite; its beds are vertical; and it is not until we descend towards Obergestelen, that we see, though in an indistinct manner, the strata dipping to the north. This northern inclination is observable in a more distinct manner at many points of the pass of the Furka, and in the Galenstock, as seen from the summit of the Col of the Grimsel.

De Saussure, and, after him, Necker, have recognised this fan-shaped structure in the group of Mont Blanc; Escher de la Linth, sen., observed it in St Gothard; and, lastly, it is displayed in a striking manner in the beautiful sections which M. Lardy has published of the rocks of St Gothard.

*Flysch, &c.*—The rocks which occupy the space between the different groups are pretty generally distinguished by their regularity. The prevailing rock is the *Flysch*, that is to say, a grey or black slate, effervescing with acids, often siliceous and passing into a dark-coloured sandstone, and of a large slaty structure; at other times very calcareous, and so much so, as to deserve the name of calcareous slate. All these varieties of flysch alternate with one another, and somee times contain large calcareous or dolomitic beds, which are frequently accompanied by masses of gypsum. In the Grisons they contain fuci, which seem identical with *F. intricatus* Brongn.; also belemnites, which it has not yet been possible to identify; and rings of pentacrinites. The belemnites are pretty frequent in the slates of the Nuffenen. M. Lardy possesses an ammonite from the slates of Salvent, which is between the group of Mont Blanc and that of the *Aiguilles Rouges;* and the same species occurs in the middle or coralline jura strata. Impressions of ferns, which cannot be distinguished from those of the coal, have been met with in slates and sandstones on the western side of the *Dent de Morcles,* on the *Col de Balme,* in the Tarentaise, and in Dauphiny; and at several places in these districts beds of anthracite are regularly mined.

When the central mountain masses or groups are far apart, as, for example, is the case in the Grisons and Valais, the flysch acquires an extraordinary development, and itself forms high chains and large mountain masses. It is subjected at the same time to marked transformations, which seem to be intimately connected with the appearance of the serpentines, and which in many cases render the mineralogical determination of the rock a matter of great difficulty. The colour of the slates and of the sandstones passes into green; talc and chlorite make their appearance; the mass gradually passes into talc or chlorite slate; the stratification disappears, and diorites and spilites present themselves, most frequently without its being possible to trace a limit between the green slates, the aphanistic rocks, the serpentine, and the gabbro; and nevertheless we can easily assure ourselves on the road of St Bernard, in the valley of Bagnes, and in most of the southern valleys of the Valais, as well as in Oberhalbstein in the Grisons, that thes green slates, so extensively distributed, cannot be separated from the ordinary grey flysch. It is to this formation of green slates that the *pierres ollaires* belong, which are quarried at so many points in the Valais and in the Grisons.

When the central masses are very close to one another, as is the case with those of the Finsteraarhorn and the St Gothard, the flysch is reduced to a narrow zone, and even disappears entirely, or at least is only represented by some detached portions in the mica slate and gneiss. In this case the limestone is generally converted into white or coloured marble, or into dolomite, and the slates become shining and acquire all kinds of tints, such as red, brown, green, &c.

It is difficult to define the relations which subsist between the flysch and the rocks of the central masses. Generally we see the flysch and a greyish limestone rising from beneath the central masses, and thus forming the lowest portion visible of the Alpine system. Above the flysch there lie mica-slates or gneissitic quartzites, which often occupy an extent of several leagues ; and above them comes the true gneiss. The stratification, at first parallel to that of the flysch, gradually becomes inclined and then vertical, and finishes by acquiring a totally opposite inclination on the opposite side of the group, where again we find appearing, under the last crystalline-slates, the marl-slates and the ordinary grey limestones. Most frequently, however, the limit between the slates of the flysch and the mica-slates is so vague that it is impossible to say where the one series terminates and the other begins ; the flysch generally becomes shining, it assumes varied colours, and we perceive in its interior, masses of true mica-slate and quartzite ; we believe ourselves already surrounded on all sides by rocks belonging to the central masses, when suddenly the flysch and the calcareous slates make their appearance anew, and often it is only after having walked for whole hours amidst these alternations, that we are assured of having really entered the domain of the crystalline rocks.

*Limestone and Slate.*—The state of matters is different at the contact of the central masses with the large deposits of limestone and slate which surround them on the north. The rock which touches the secondary beds is generally not mica-slate, but an imperfectly slaty quartzite, sometimes containing felspar, and whose stratification is distinct only towards the interior of the central mass, where it becomes transformed into a true gneiss or into gneissitic granite ; its beds then become vertical, and even dip to the south. The sedimentary rocks are in discordant stratification with this quartzite ; they are inclined to the north, and their beds, which follow the external slope of the gneiss, dip at first under very high angles ;

afterwards losing their inclination, they become horizontal in proportion as their distance increases from the central mass; and then they rise to the north, so that, in fact, they assume the form of a vast basin with a flat bottom. It results from the discordant stratification of the gneiss and the limestone, that these two rocks are generally separated by a valley, one of whose walls ascends towards the icy regions of the central mass, offering, by means of deep gorges, outlets for its glaciers and for its torrents, while the opposite wall presents the vertical precipices of the calcareous deposits. The extent of these valleys is very variable; sometimes they are prolonged from the highest summits down to the inhabited districts, and down to the level of our lakes, following all the sinuosities of the central mass. Thus, therefore, if a traveller, following the northern limit of the group of the Finsteraarhorn, were to proceed by the valley and the glacier of Lötsch towards the valley of Gastern, to traverse the glacier of Tschingel, in order to reach the bottom of the valley of Lauterbrunnen, to ascend the high valleys which separate the Jungfrau from the Silberhorn and the Mönch from the Eiger, afterwards to arrive at the *névé* and the lower glacier of Grindelwald, to mount the *Col d'Urbach* by the glacier of Rosenlaui, to descend the valley of the same name as far as Hoff, then to go up the valley of Gadmen, and across the glacier of Wenden, proceeding along the southern slope of the Titlis, next to pass into the valley of the Reuss, and thence into the valley of Maderan, in order to arrive at the eastern limit of the central mass of the Finsteraarhorn, in the neighbourhood of the Tödi,—such a traveller would have almost constantly on his left hand vertical walls of limestone, often several thousand feet in height, and on his right the central group, sometimes crowned by *nevés* and glaciers, sometimes covered with forests and pastures, and rarely presenting inaccessible escarpments. Behind this belt, which is developed round the central mass like the walls of a crater around a central volcano, we frequently remark traces of a second and a third rampart, exhibiting the same inclination of the beds, that is to say, presenting its vertical walls to the central mass, and dipping in the opposite direction. The escarpments of the Gemmi belong to such a secondary rampart, and the baths of Leuk are situated between the two chains of sedimentary rocks. At the eastern extremity of the group of Mont Blanc, from Saillon to Sion, we can reckon four or five parallel chains of limestone and of flysch, all of which have their escarpments turned towards the central mass, while they present a gentler declivity to the east.

We frequently see appearing, at the base of the first limestone rampart, gneiss and quartzite, which rise sometimes to a very great height, as, for example, at Hoff, near Imgrund, at Gadmen, at Maderan, &c. and which plainly prove that the origin of the valley is here more recent than the event, whatever it may have been, which placed the calcareous beds in their present relations with the gneiss, and that, therefore, the formation of the valley cannot be attributed to a plutonic *soulèvement* of the gneiss, which would have caused the upper limestone covering to disappear. The same phenomenon is more distinctly observable on the opposite side of the valley; we there see calcareous masses of more than a thousand feet in thickness, covered by the same gneissitic quartzite which serves as their base; and, when we follow the transverse valleys which penetrate into the central mass, we see the limestone entangled in the gneiss, and forming wedges of many leagues in extent (see the section, Plate I.). This is what takes place in the Mettenberg, and in a more striking manner in the Laubstock and Pfaffenkopf, on the two sides of the entrance to the valley of Guttanen. It is evident that before the formation of the valley, these wedges of limestone communicated with the limestone of the other wall, and that they were then, as at present, surrounded by the gneiss. The old surface of the ground reached at that period a great elevation above the valleys of the present day, and these valleys, which in a great measure seem to owe their origin to subsidences, have crossed the gneiss and the limestone without changing the relative position of these two rocks. It is sufficient to witness these phenomena in nature to be convinced of this. But further, the magnificent sections of the valley of the Rotthal, and of the Col d'Urbach, furnish us with the proof of this opinion; for there, where the subsidence perhaps encountered difficulties in consequence of this entanglement of the limestone and the gneiss, we see in reality the wedges of limestone which penetrate the gneiss, adhering to and incorporated with the main mass of the limestone.

The determination of the age of the sedimentary beds, situated to the north of the Alps, presents great difficulties, but nevertheless these difficulties belong rather to the details than to the general features. We know that in Switzerland, at least, the most ancient sedimentary deposits contain belemnites, particular species of ammonites, and pentacrinites, and that therefore they cannot be of more ancient date than the lias. We know, moreover, that the upper masses of our sedimentary masses belong to the chalk, and frequently the limit between the cretaceous strata and the Jurassic formation can be traced with perfect certainty.

*Mixed Formation.*—Immediately above the gneissitic quartzite we generally find a series of peculiar rocks, which we propose to term the Mixed Formation (*terrain mixte*). The thickness of this series is frequently not greater than a few fathoms, but in other localities it amounts to several hundred and even several thousand feet. Amongst these beds we often meet with quartzites which it is very difficult to distinguish from the gneissitic quartzites, if indeed they are really different; but they have one peculiarity, that they dip to the north like all the other rocks of this formation. Dolomitic limestones, which are either compact or cellular, also occur, and these are identical with the *carnieule* (*Rauchwacke*) which accompanies our gypsums, and have a yellowish tint passing into red and brown. Red argillaceous slates likewise present themselves, with which are associated argillaceous or talcose conglomerates. The latter sometimes constitute entire mountains. Finally, above these last we find red or blackish green ferruginous oolites, containing magnetic iron ore, iron glance, and pyrites. In general, this whole formation is very ferruginous; there are no conglomerates which do not contain iron, and several spots are shewn where it was formerly attempted to carry on mining operations. The oolitic bed affords many petrifactions, especially ammonites and belemnites.

*Limestone of the High Alps and Black Slate.*—A very thick and uniform limestone formation, the limestone of the high

Alps (*Hochgebirgskalk*), reposes on the mixed formation; it generally presents enormous escarpments turned towards the gneiss masses, and the large limestone wedges which are entangled in the mass of the gneiss are generally formed of it. The beds of limestone are distinctly separated, and the rock often passes almost into slate. Its colour varies from grey to black; it is as friable as glass, and, with the exception of some belemnites, it contains no petrifactions. In the Hasli valley, this limestone formation is covered by a thick series of brilliant black slates, which, at Planplatten, above Meyringen, contain considerable deposits of red oxide of iron and iron in grains. In the vicinity of these ferruginous repositories, the slates frequently include prominent nodules and ammonites, which evidently belong to different members of the oolitic formation, as, for example, at Erzeck, and near Unterheid, opposite Meyringen. Belemnites are of rarer occurrence.

*Chalk Series.*—The cretaceous formation succeeds these black slates, and when these are awanting, it follows the limestone of the high Alps. As in Southern Europe, this formation is distinguished by peculiar characters, and likewise by the extent and thickness of many of its subdivisions, which confer on it considerable importance in the composition of the calcareous Alps. A fact worthy of attention is, that frequently, and more particularly in the first limestone chain, the Alpine limestone is separated from the chalk by a series of rocks having the greatest analogy to the mixed formation of which we have spoken, and consisting of beds of quartzite, of brilliant variegated slates, and of crystalline and dolomitic white limestones. This is observable on the Gemmi above Kandersteg, at the bottom of the valley of Lauterbrunnen, on the Wengern-Alp, and on the Faulhorn; and perhaps we may include under the same head the beds of iron in grains, and the black slates of Planplatten.

The arenaceous black limestones, the quartzose sandstones, and the rough slates, which repose on the limestone chains that are nearest to the gneiss groups, and which form the largest portion of the mountains situated on the exterior of these latter, seem to belong to the most recent layers of the Alpine cretaceous formation. The most abundant fossils there are nummulites, and species of cerithium, ampullaria, ostrea, turbinolia, &c. Ammonites and belemnites are, however, almost entirely awanting. The nummulitic limestone forms the superior mass of the Dent de Morcles and of the Diablerets, and continues along the northern side of the chain which is crossed by the Cols of the Sanetsch, the Rawyl, and the Gemmi, as far as the upper part of the lake of Thoune. Some isolated nummulites are met with in the chain of Brienz; and an accumulation of fossils similar to those of the Diablerets occurs on the crest of the chain of Gadmen (*Gadmenflue*), of which the Titlis forms a part. Lastly, the nummulitic limestone is largely developed in the mountains of Schwytz and of Glaris, and in the chain which separates the canton Glaris from the valley of the anterior Rhine.

*Alpine Macigno.*—A deposit of grey slates, which is frequently very thick, extends in many places over the nummulitic limestone, and of itself forms separate masses, which are generally wooded or covered by pastures to the summit. In a mineralogical point of view, this rock does not differ in any respect from the true flysch of the Alps, of the Valais, and of the Grisons. The only organic remains it contains, are, moreover, fuci, similar to the *Fucus intricatus* of A. Brongniart, and we might consequently designate it under the name of flysch, in so far as that term merely implies a petrographical character. But to admit that this slate, which reposes on the nummulitic limestone, is of the same age as the flysch which occurs between the central masses, would be opposing probabilities to too great an extent. In order to avoid all confusion, and to prejudge nothing, we shall call this rock *Macigno alpin,* remarking at the same time, that we recognise it neither mineralogically nor geologically as the analogue of the macigno of the Apennines. A deposit of this slate or macigno, having a breadth of several leagues, extends behind the chain of Brienz, along the western side of the Lake of Sarnen, towards Alpnach, and occupies a large portion of the Entlibuch and of Obwalden. Other deposits exist in the neighbourhood of Beckenried, and to the south of Einsiedeln. Lastly, this formation attains a large development in the south of the canton of St Gall, and in the Prettigau.

In no district, however, are the nummulitic limestone and the macignos better developed than in the most northern chain of the limestone Alps, which, traversing the cantons of Apenzell, of Schwytz, and of Unterwalden, afterwards includes Mont Pilate, the Schratten, and the Hohgant. It is this same chain which is prolonged to the west of the lake of Thoune by the Gemmi, the Diablerets, and the Dent de Morcles, beyond our frontiers to the Buet, and the mountain of Fis, so celebrated for its fossils, and then across Savoy into Dauphiny; and in this prolongation, the inferior beds of chalk can be more easily determined than in the interior of the Alps, where they are generally very deficient in fossils.

*Seeven Limestone.*—We find under the nummulitic limestone, and sometimes replacing it, a thick limestone formation, which has a character depending on the district, and forms the upper part of the chains. In the cantons of Appenzell and of Schwytz, M. Escher has given to this formation, which seems to be awanting in a portion of Switzerland, the name of *Calcaire de Seeven.* It is for the most part compact, of a grey or red colour, destitute of organic remains, and contains thin undulating argillaceous lamina. The lower part of this deposit generally forms those vast corroded and fantastically furrowed rocks, which in Switzerland are known under the name of *Karrenfelder*, and in which we find numerous organic remains, especially dicerates, a particular species of hippurite (*H. Blumenbachii*), and some large gasteropods allied to the tornatellæ. It is probable that this division corresponds with the *calcaire à hippurites* of the south of France.

*Glauconite.*—Below this limestone occurs the bed with inocerami, which frequently has a thickness of only a few fathoms, but is remarkable for the quantity of fossils it contains, which fossils are for the most part identical with the species of the *Perte du Rhône* and the glauconite of Rouen.* The rock is a blackish siliceous limestone, filled with greenish grains, intimately mixed with the mass, which presents at the surface a reddish tint, the result of oxidation. In the canton of Appenzell this bed is seen lying under the Seeven limestone. In central and eastern Switzerland traces of it have been seen in several places; but observers have not yet succeeded in following it in a constant manner; it appears distinctly only on the eastern frontier of Switzerland, on the northern side of the Buet, on the Col de Coux, whence it has been traced without interruption by Sixt, Fis, the Reposoir, &c., along the French Alps, as far as the sea. The varied and numerous fossils it contains have caused geologists to single it out with preference from all the other members of the chalk series.

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* The most characteristic of these fossils are, *Inoceramus concentricus*, Park.; *Inoceramus sulcatus*, Park.; *Ammonites inflatus*, *Ammonites subcristatus*, Brong., Sow.; *Turrilites Bergeri*, Brong.; *Trochus Gurgitis*, Brong.; *Trochus cirroides*, Brong.; *Cassis Avellana*, and *Micraster minimus*, Ag.—E. Desor.

*Neocomian Formation.*—The Alpine Neocomian formation, or *Calcaire à Spatangus* (*Holaster* or *Toxaster*) *complanatus*, makes its appearance under this glauconite at many points in central Switzerland, and immediately under the hippurite limestone, when the glauconite is awanting, as, for example, in the Chain of Pilate and the Hohgant. It generally has a considerable thickness. A grey limestone, filled with nerineæ and pterroceræ forms it upper bed in the canton of Appenzell. For the most part, however, this division presents itself in the form of a thin-bedded black limestone, which is marly or sandy, and which includes the characteristic petrifactions of the marls of Neufchatel; among others, *Holaster* (*Toxaster*) *complanatus*, *Exogyra Couloni*, *Ostrea carinata*, *Terebratula depressa*, *Terebratula biplicata*, &c. It does not appear that any older beds of the chalk series exist either in the Alps or in the Jura.

*Tertiary Formations.*—In central Switzerland the tertiary deposits abut directly against the external chains formed of the Alpine chalk, such as the chains of the Pilate, the Sentis, and the Ralligstöcke. It is not thus to the west of the lake of Thoune. In the environs of Chambery, where the great valley of the Swiss molasse seems to open, we see a large branch of the Jura system separate itself from this last, follow the direction of the Alps as far as the lake of Thoune, and attach itself to the extreme Alpine cretaceous chain. By this junction the Alpine character becomes confounded in the most striking manner with the Jurassic character. The different divisions of the lias, of the inferior oolite, of the coralline and Portland oolite, are determined by their organic remains, and are better separated than at any other point in the Alps; the upper Jura formation in particular, which appears to be completely a stranger to the internal and oriental Alps, here attains a remarkable development, and forms several parallel chains. We remark also in many of these chains a tendency to become arched, whereas in the Alpine chains the beds are only prolonged on one side. The physiognomy of the valleys reminds us of the softer and more uniform character of the Jurassic valleys; but nevertheless the forms are more abrupt, the chains are not only cut transversely by *cluses*,* but they also contain numerous oblique ravines, which impart to these valleys a more picturesque and more varied aspect. The limestone assumes the more the black tint of the limestone of the Alps the nearer it approaches the central masses of the Alps properly so called. The molasse, which penetrates into all the large Jurassic valleys, is entirely awanting in the Alpine Jura as well as in the Alps. This mixed character is exhibited still more distinctly in the cretaceous formation; the nummulitic limestone, so largely developed in the Alps and in a large portion of Southern Europe, does not exist at all in the Alpine Jura. The Hippurite limestone, the bed containing Inocerami, and the Neocomian strata, do not penetrate into the Chablais, and are entirely strangers to the interior of the Swiss Alpine Jura (district of En-haut, Gessenay, and the Simmenthal). They are there replaced by the flysch, which attains a great development, occupying the bottom of the enlarged valleys, rising to a great height on the flank of the limestone chains, and forming even particular chains, whereas it is completely awanting in the Jura. The chain of the Niesen, the mountains of the Simmenthal, and the district of Gessenay, are composed chiefly of flysch. The calcareous masses of these same valleys, such as the Tour d'Ay and Mayen above Aigle, and the Cornettes in Chablais, are of the upper Jura formation. The Stockhorn and the Male-son, the Dent d'Oche and the Mole, belong to middle Jura, and partly to the lower Jura formation. The middle Jura and the lias likewise appear on the northern side of the Alpine macigno chain, on the Gurnigel, at the Beira, on Mont Pleyau, and in the Voirons.

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* The geologists of the Jura give the name of *cluses* to those cuts which cross a mountain chain from one side to the other. When the rent is limited to one of the flanks, it is termed *ruz*.—E. D.

In western Switzerland and in the Chablais, the molasse is in contact with these arenaceous and calcareous chains of the Alpine macigno and the middle Jura; and, as in the rest of Switzerland, it touches the cretaceous chain of limestone containing spatangi and hippurites. Similar to this latter, the chain of Alpine macigno, together with the limestone masses (middle jura of the Stockhorn), dips abruptly to the south and south-east towards the high Alps. But wherever an opportunity occurs of examining the junction of the tertiary formation with the secondary deposits, between Vevey and the valley of the Rhine, we can assure ourselves that the molasse extends in conformable stratification under the latter, so that the more recent deposits are here inferior to the more ancient; or rather the two are in such proximity, that one would say that the masses of limestone and of macigno have been violently pushed against the masses of the molasse. This supposition may explain the singular relations of contact where an upturning cannot be admitted, for a fault without lateral pressure is not sufficient to account for them.

The southern inclination of the tertiary deposits continues nearly to the distance of a league from the external chain of the limestone or macigno, where it is replaced by a northern inclination, which becomes more and more gentle the farther we remove from the Alps, until the stratification forms only a very acute angle with the horizon. At the same time the *relief* of the country presents a series of terraces more and more soft in their outline, till at last they pass into an undulating plain, and the valleys of elevation are replaced by valleys of erosion. In the immediate vicinity of the Alps the tertiary deposits rise to the height of 5000 or 6000 feet and more, as in the Bæuchlen, the Entlibuch, the Rigi, and the Speer; but gradually hills of more than 300 feet become rarer, and begin to fix the attention; still farther on it would be difficult to find, between the Uetliberg near Zurich, and the Frienisberg near Aarberg, a third hill of a greater height than 1000 feet above the level of the plain. On the other hand, undulations and *plateaux* of some hundred feet in height, and valleys of erosion of an equal depth, are the most frequent phenomena as far as the Jura range.

*Molasse.*—The tertiary formation of Switzerland is essentially composed of a marly sandstone, the *molasse*, whose hardness goes on diminishing from the Alps to the Jura. Immediately at the foot of the Alps it is an extremely compact sandstone; in the central part of the Swiss plain, it forms an excellent building-stone; and in the neighbourhood of the Jura mountains the entire mass is nothing but a loose sand. Near the lake of Geneva, and in the cantons of Aargau and of Zurich, we find, in the lower beds of the molasse, deposits of lignite, accompanied by a bituminous calcareous marl, which often contains a large number of fresh-water shells. The lignite itself and the molasse contain teeth and bones of land-animals, which are characteristic of the upper tertiary formation, and also remains of palms. The upper part of the molasse is of marine origin; and it may be said that the entire beds consist of internal casts or debris of shells, more rarely of entire specimens of marine shells, of which the determinable species are identical with those of the subapennine hills.*

*Nagelflue.*—To the sandstone of the molasse we find frequently added, in the neighbourhood of the Alps, conglomerates of rounded pebbles, known under the name of *nagelflue* or *gompholites*. The subdivision, thickness, and even composition of this rock are very variable. Where the cretaceous chains are in immediate contact with the molasse, as in eastern and central Switzerland, the nagelflue appears in much more considerable masses, and over much larger spaces than in western Switzerland, at the foot of the Alpine Jura. The pebbles of which it is there composed, are, as in the Rigi and in eastern Switzerland, essentially limestones and sandstones, which correspond with the nearest rocks of the Alps; nevertheless there are also mixed with these, granites and other rocks whose origin is unknown.

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* Notwithstanding this identity of fossils, authors have persisted in separating the Swiss molasse from the subapennine formation, and in identifying it with the formation of la Superga, Bordeaux, and Dax, whose fossils differ as much from those of the molasse as is possible in formations so nearly approaching each other. Whatever name may be given to these different subdivisions of the upper tertiary formation, I hope that the Swiss geologists who occupy themselves with the study of the paleontology of the molasse will continue to protest against this factitious arrangement.

In the nagelflues of the canton of Berne, which form several chains of hills whose beds dip, sometimes to the south, sometimes to the north, it is these rolled masses, foreign to the Alps, which predominate; in the nagelflue of environs of Thoune, whose beds dip to the south, we find, in particular, red porphyries and granites of all kinds, such as occur in the great porphyry chains. Similar rocks present themselves in the Black Forest, and at the southern base of the Alps. Near Thoune, another kind of pebbles is mixed with these rocks, and predominates especially in the nagelflue of the Belpberg, and of the Emmenthal, which is horizontal or has a northern inclination. These are fragments of serpentine and gabbro, rocks which only exist *in situ*, so far as we know, in the southern part of the Grisons, also of compact serpentines containing diallage, several other species of gabbro, granites, green porphyries, green or violet-coloured slates, and aphanites. The Emmenthal likewise contains spilites or amygdaloidal rocks, and variolites. In a small district to the north of the Emmenthal, pebbles of quartz and the debris of a hornblendic rock predominate. The sand brought down by the torrents of this district contains grains of gold with much magnetic iron, garnets, and other minerals. Where are we to seek for the beds of porphyry, of serpentine, and of auriferous hornblende, which have furnished those enormous quantities of rolled stones? In what manner, and by what agent, have these pebbles been accumulated at the base of the Alps, in masses sufficiently large to appear considerable even in the vicinity of those elevated summits? These problems are still unsolved. To suppose that the pebbles have come from the interior of the earth, and that their considerable masses, produced during the long period which corresponds to the last convulsions of the Alpine system, cover the beds whence they were derived, or tha [illegible] very beds are partly destroyed, is too arbitrary a mode of getting rid of the difficulty, and yet it would not be easy in the present state of our knowledge to find a less daring solution.

*Diluvium and Erratic Blocks.*—The surface of the molasse valleys and of the Swiss plain is covered by a bed of gravel and of sand which sometimes attains a thickness of more than 100 feet; this is the *diluvium.* In its nature, this deposit corresponds exactly with the deposits brought down by the torrents of the Alps. The predominating rocks are limestones and Alpine sandstones, mixed with rolled masses of nagelflue. The pebbles are always rounded, but rarely attain the size of a man's head. It is in this diluvium, and even deeper than it, that the present rivers have hollowed out their beds. The superimposed terraces indicate alternations of the epochs of repose and of activity.

These large masses of gravel are covered by a more recent diluvium, generally unstratified, containing large and small pebbles, which are both round and angular, and also blocks of several fathoms in diameter, imbedded in a sandy clay. The largest blocks are sometimes isolated, sometimes united in groups; the one series rounded and blunted, notwithstanding their large size, and the other more or less angular. This recent diluvium sometimes abuts against the molasse hills, and sometimes forms dykes or elongated hills from 20 to 100 feet high, generally running parallel to the foot of the slopes or following a transverse course in the valley. The only difference observable between the blocks of this recent diluvium and true erratic blocks, is that the latter are isolated and free, whereas the first are imbedded in the sand or the gravel. There is a striking resemblance between the dykes formed of transported materials and the descriptions given us of the *Osars** of Sweden. Now, if the glacial theory, according to which some celebrated geologists believe that this phenomenon ought to be explained, be really, of all the hypotheses hitherto proposed, that which harmonizes the best with facts, the same theory, in my opinion, ought to be applicable to the phenomenon presented by Sweden.

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* Osars, to which the attention of the geologists was first directed by M. Alexander Brongniart, are long lines of transported materials having the form of dykes or banks generally running N.N.E. and S.S.W.—E. D.

### *General Conclusions.*

It may be seen from what has been said, that we are still far from being acquainted with all the phases of the history of the formation of the Alpine system. Nay, it is probable that science is yet ignorant of the very principles upon which an explanation of the most characteristic features ought to be based. It is, therefore, not astonishing if the geologist who attempts to give a summary of the precise ideas acquired from the time of Saussure to the present day, should at every step be afraid of advancing too far, or of being forced to retract what he has asserted, owing to the insufficiency of his facts. After thirty-six years of travels and studies, the immortal De Saussure himself wished to have the power of recommencing the examination of the Alps, aided by the experience which he had acquired during a life devoted to this subject. Since then more than half a century has elapsed, and we still find ourselves embarrassed when we are asked for an abstract of the chief facts regarding the structure of the Alps.

The documents which nature affords us, do not reach beyond the epoch of the lias. We possess no knowledge of the condition of the surface of the earth in the domain of the Alps at preceding epochs.

During the period which elapsed between the deposition of the lias and that of the last portions of the chalk strata, this portion of the surface of Europe does not appear to have been subjected to derangements in its relief. Everywhere the lias series is arranged conformably with the macigno, and the whole mass forms a whole which it is almost impossible to divide. Nevertheless this series of jura-cretaceous deposits of the Alps and of all the south of Europe, differs considerably from that which corresponds with it in age in northern Europe. One would say that they were sedimentary deposits formed in different seas and under different conditions.

The most important events which have modified the surface of Switzerland took place between the epoch of the deposition of the Alpine macigno and that of the molasse. We find the nummulitic limestone and the macigno on the summits of the *mountains* and in the bottoms of the valleys, while there exists *in such situations* no trace of the molasse. The Alpine system must therefore have emerged subsequently to the deposition of this latter formation.

The agencies, doubtless of very long duration, which have operated on the Alpine sedimentary formations, have modified the original nature of the rocks, and transformed them either into magnesian rocks (dolomite, talc-slate, chlorite-slate, and serpentine), or into micaceous and felspathic rocks (mica-slate, gneiss, and granite).

These modifications were accompanied by an elevation of the surface and a general change of position of the beds. It is in the regions where the metamorphic agencies have operated with the greatest energy, that the formation took place of those enigmatical fan-shaped arrangements, and of those entanglements of felspathic, quartzose, and calcareous fossiliferous rocks.

The modifications as well as the breaking up of the surface have operated in various directions and at different epochs, the consequence of which has been that the external direction of the chains does not generally correspond with the direction of the beds. There are not only longitudinal and transverse valleys, but also longitudinal and transverse chains, diagonal valleys and diagonal chains.

The chief directions in Switzerland are: 1. From W.S.W. to E.N.E. (more exactly from W. 23° S. to E. 23° N.), parallel to the principal direction of the Alps. Examples: the chain between the Diablerets and the Altels; the great valleys of the Valais and of the *Vorder* Rhine.

2. From S.W. to N.E. (more exactly from W. 37° S. to E. 37° N.), parallel to the direction of the Alps of Savoy. Examples: the central group of the Finsteraarhorn and that of Saint-Gothard; the upper Valais; the lake of Brienz.

3. From S.S.W. to N.N.E. (more exactly from W. 53° S. to E. 53° N.), parallel to the system of the Alps of Dauphiny. Examples: The group of Mont Blanc; the Italian lakes; the depression between Brünig and Küssnach; the eastern chain of the Niesen.

4. From S.S.E. to N.N.W. (more exactly from W. 60° N. to E. 60° S.), parallel to Monte-Viso according to M. Elie de Beaumont. It is the direction which prevails in the beds and

intermediate valleys between the northen part of the St Gothard and the upper Engadine.

The effects of the dislocations produced in these directions and in many others, are limited to the zone of the Alps, and do not even reach their northern and southern limits ; nevertheless, the formation of the valleys parallel to the Alps, and even of the Alpine valleys in general, appears to be more recent than the formation of the central masses and of the chains having inclined beds, for they cut obliquely the axis of these *soulevements.*

At the end of this period of convulsion, we find the domain of the Alps, and the low plain which borders it on the north, elevated above the level of the sea ; but this plain was in a great measure covered with pools and marshes of fresh water, in which the terrestrial animals of the period were destroyed. It was then that there commenced the deposition of the molasse, of that product of erosion and of the friction of the rocks against one another, on the northern edge of the Alps. At that period the northern side was probably flanked, as the southern is at the present day, by a belt of porphyritic and serpentinic rocks which have partly furnished the materials of the nagelflue and of the molasse.

The formation of the upper deposits of the molasse is the result of a new invasion of the Swiss plain by the sea ; but the organic remains of that period do not the less prove the existence of lakes of fresh or brackish water at the side of the sea shore. The accumulation of the debris of rocks on the northern edge of the Alps under the influence of a more violent action, and the dispersion of a fine sand over the whole plain, terminate the epoch of the nagelflue and of the molasse.

A new dislocation of the Alpine surface took place between the formation of the molasse and of the ancient diluvium, a dislocation which appears to have been followed by the formation of many of the valleys of subsidence in the Alps. In consequence of this last soulevement, the secondary Alpine formations were pressed against the tertiary strata, and the latter, being elevated and broken up, acquired a more or less considerable inclination.

By placing this movement in connection with a general *soulevem*[illegible] the Alpine system above the level of the sea, we can partly explain the valleys of erosion of the molasse, which would thus be hollowed out by the retreat of the waters.

A long period of repose undoubtedly followed this retiring of the water; and during this time all the low regions of the molasse were covered by the Alpine alluvial matters which are designated by the term ancient diluvium. The level of the ancient diluvium streams which, in many places, is about 200 feet above the present level of the rivers, is in favour of a more elevated level of the lakes into which these rivers flowed. This period was terminated by the deposition of the recent diluvium, and the dispersion of the large erratic blocks. If we attribute this dispersion to the movement of the ice which transported the blocks and gravel that surround them to great distances, we must also admit that during all this period, necessarily of very long duration, the surface of Switzerland, and consequently that of a large portion of Europe, were subjected to a climate nearly such as is presented to us by Terra del Fuego or the Antarctic continent.

At a still later period, intermittent movements of the surface produced the relative depression of the basins which receive our rivers, and the currents of water hollowed out their beds down to their present level, in the two diluviums and the molasse.

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## *On the structure of the Intestinal Villi in Man and certain of the Mammalia, with some observations on Digestion, and the Absorption of Chyle.* By JOHN GOODSIR, Esq., M.W.S. Surgeon, and Conservator of the Museum of the Roy. Coll. Surgeons, Edinburgh. Communicated by the Author.

Mr Cruikshank, in treating of the orifices of the Lacteals and Lymphatics,[*] states that he and Dr William Hunter observed the openings by which the lacteals communicated with the cavity of the gut in portions of the intestine of a woman who died after eating a hearty supper. The two preparations of the intestine on which these celebrated anatomists made their observations came into the possession of the College of Surgeons in Edinburgh, as part of the collection of the late Sir Charles Bell. When employed lately in removing these interesting specimens into fresh spirits, I availed myself of the opportunity of examining into the nature of the appearances described and figured by Mr Cruikshank.*

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[*] *William Cruikshank.* The Anatomy of the Absorbing Vessels of the *Human Body, 2d Ed. 1790,* page 56.

I removed one of the villi from Mr Cruikshank's preparation,† placed it between two glass plates, and had no difficulty in recognising what had been described and figured by the original owner of the preparation. With a low power the extremity of the villus appeared bulbous and opaque. With a higher power I observed that this opacity was due to the existence, at the extremity of the villus, of a number of vesicles of different sizes. The larger vesicles were pretty uniform in size, and about twenty in number. The smaller were of different sizes and more numerous, and appeared gradually to pass into the granular looking tissue of the attached extremity of the villus. No blood-vessels could be detected, but along the neck of the villus distinct traces of two or more opaque lacteals were visible. The vesicles and the lacteals, when viewed by transmitted light, were of a light brown colour; but when examined as opaque objects, against a dark ground, they stood out of a dead white appearance, contrasting strongly with the semi-transparency of the surrounding tissue. Repeated examinations of these preparations satisfied me that Dr William Hunter and Mr Cruikshank were quite correct in describing and figuring radiating lacteals within the villi, but that they were led into error in describing those vessels as opening on the free surface of the gut, partly by imperfect instruments and methods of observation, partly by the general prejudice of the period in favour of absorbent orifices. I also satisfied myself of what appeared highly probable from the commencement of the observations, that the villi, when turgid with chyle, were destitute of their ordinary epithelial covering. This circumstance I could not avoid connecting with the fact of the stomach throwing off its epithelia during the process of digestion. I determined, therefore, to investigate the process of absorption of chyle in fresh subjects, as the facts exhibited in Mr Cruikshank's preparations indicated the probable existence of complicated processes going on in villi during digestion. The analogy of the vesicular bulbous extremity of the villus, to the spongiole of the vege-

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souleven *Plate* 11, figs. 2, 3.

† xiii. 4 N. 49, Bell Catalogue.

table, forced itself upon me; and the existence of milky chyle, within closed cells, led me to anticipate an explanation of some of the phenomena of digestion.

A dog was fed with oatmeal, milk, and butter. Three hours afterwards he was killed, and a cord thrown round the root of the mesentery. The lacteals became more turgid, and the gut, when opened, was found to be full of milky chyme, with an admixture of thin brownish fluid of a bilious appearance. The milky matter was situated principally towards the mucous membrane; the brown fluid occupied the cavity of the gut.

The white matter was found to consist of a transparent fluid, with a few oil globules, and numerous epithelia.

Some of the epithelia I recognised as those which cover the villi. They were pointed at their attached extremities, flat at the other. (Fig. 1, Pl. I.) Many of them were single, others were united in bundles, adhering principally by their flat or free extremities, as if a fine membrane passed over and connected the edges of their extreme surfaces. (Fig. 2, Pl. I.) Occasionally these epithelia presented a distinct nucleus; but generally, and whether single or in bundles, they exhibited in their interior a group or mass of oil-like globules, which, when viewed as opaque objects, had a peculiar semi-opaque or opalescent appearance*. (Fig. 3, Pl. I.) Others of the epithelia, contained in the chyme, were prismatic, single, or in columns. (Fig. 4, Pl. I.) They were the lining epithelia of the follicles of Lieberkühn, and presented the usual nuclei.

The mucous membrane displayed the villi turgid as if in a state of erection, and, as I had anticipated, naked or destitute of epithelia, except at their bases where a few still adhered. Each villus was covered by a very fine smooth membrane, which from its free bulbous extremity, passed on to its sides, and became continuous with what I have elsewhere denominated the primary membrane (Trans. Roy. Soc. Ed. 1842) of the follicles of Lieberkuhn. (Fig. 8, Pl. I.) These villi when removed from the mucous membrane, and examined with $\frac{1}{8}$ inch magnifier, were semi-transparent, except at their free or bulbous extremities, which appeared both by direct or transmitted light white and opaque; under higher powers they exhibited appearances represented in fig. 6, Pl. I. The summit of the villus, somewhat flattened, was crowded immediately under the membrane before mentioned, with a number of perfectly spherical vesicles. These vesicles varied in size from 1000 to less than 2000 of an inch. The matter in their interior had an opalescent milky appearance. Towards the body of the villus on the edges of the vesicular mass, minute granular or oily particles were situated in great numbers, and gradually passed into the granular texture of the substance of the villus.

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* Is this appearance due to a partial absorption of chyle by these protective epithelia ?

The trunks of two lacteals could be easily traced up the centre of the villus, and as they approached the vesicular mass they subdivided and looped. In no instance could one of these lacteals be traced to any of the spherical vesicles, nor could any direct communication between the structures be detected. The bloodvessels and capillaries, with their columns of tawny blood disks, could be seen passing in radiating lines and in loops across the villus, immediately under the fine membrane already mentioned. This membrane, perceptible on the body and neck of the villus only by the smooth surface it presented, was most distinctly traced at the free extremity of the villus, as it passed from the surface of one vesicle on to that of another. The vesicles pushing the membrane forward, and grouped together in masses on its attached surface, gave the extremity of the villus the appearance of a mulberry. When viewed on a dark ground as an opaque object, the point directed to the light, a villus in this condition is remarkably beautiful, the play of the light on the surface of the highly refractive semi-opaque and opalescent vesicles, giving them the appearance of a group of pearls.

In villi turgid with chyle, which have been kept for some time in spirits, the contents of the vesicles are opaque, the albumen having become coagulated.

The villi of the rabbit exhibit similar vesicles during digestion, and I am at present engaged in preparing drawings and descriptions of these formations in the different classes of the animal kingdom.

To understand the part which the vesicles of the villus play in digestion, it is necessary to be aware of certain of the *functions of the cell,* with which physiologists are yet unacquainted.

Not only are these bodies the germs of all the tissues, as determined by the labours of Schleiden and Schwann, but as I have observed, they are the immediate agents of secretion. (Trans. Roy. Soc. Edin. 1842.) A primitive cell absorbs from the liquor sanguinis which surrounds it, and which is supplied by the capillaries, the matters necessary to enable it to form, in one set of instances, nerve, muscle, bone, if nutrition be its function; milk, bile, urine, in another set of instances, if secretion be the duty assigned to it. The only difference between the two functions being, that in the first, the cell dissolves nd disappears among the tissues, after having performed its part; in the other, it dissolves, disappears, and throws out its contents on a free surface. Now, it will be perceived that before a cell can perform its function as a nutritive cell, or as a secreting cell, it must have acted as an absorbing cell.* This absorption, too, must necessarily be of a peculiar and specific nature. It is in virtue of it, that the nutritive cell selects and absorbs from the liquor sanguinis those parts of the latter necessary for building up the peculiar tissue of which the cell is the germ. It is in virtue of this peculiar force that the secreting cell not only selects and absorbs, but also in some instances elaborates, from the same common material, the particular secretion of which it is the immediate organ. And it is by the same force that the cell becomes the immediate agent of absorption in certain morbid processes.

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* "Absorption," says Professor Müller, Baly's Translation, p. 301, "seems to depend on an attraction, the nature of which is at present unknown, but of which the very counterpart, as it were, takes place in secretion; the fluids altered by the secreting action being impelled towards the free surface only of the secreting membranes, and then pressed onwards by the successive portions of fluid secreted. In many organs, for instance in those invested with mucous membranes—absorption by the lymphatics and secretion by the secreting organs, are going on at the same time on the same surface." It appears, however, from what I have stated in the present paper, and in Trans. Roy. Soc. Edin. 1842, that Prof. Müller, and indeed all the physiologists hitherto have been in error in supposing the forces of secretion and absorption as of different and opposite tendencies,—the one attractive, the other repulsive. They are both attractive, absorption being but the first stage in the process of secretion. Secretion, in fact, differs from absorption, not physiologically, but morphologically.

What has been stated in the present paper explains also how, in the mucous membranes, "absorption by lymphatics and secretion by secreting organs are going on at the same time on the same surface." (Müller, *loc. cit.*) There is no physiological mystery in this. It depends on a morphological circumstance. The absorbing chyle cells are on the attached surface of the primary membrane—the secreting epithelia are on its free surface; the former are interstitial cells, the latter peripheral; the former cast their contents into the substance of the organism,—the latter into the surrounding medium. It may be here observed that absorption, as it occurs in the chyle vessels, takes place as in the absorption which occurs in all the secreting cells, through two structureless membranes, probably molecular in their constitution—the primary membrane and the membrane of the cell.

The primitive cell, then, is primarily an organ of specific absorption, and secondarily of nutrition, growth, and secretion.

With these few introductory observations on subjects which I shall consider more at length on another occasion, I may proceed to apply the laws of structure and function of the cell to the structure and function of the intestinal villi.

As the chyme begins to pass along the small intestine, an increased quantity of blood circulates in the capillaries of the gut. In consequence of this increased flow of blood, or from some other cause with which I am not yet acquainted, the internal surface of the gut throws off its epithelium, which is intermixed with the chyme in the cavity of the gut. The cast off epithelium is of two kinds,—that which covers the villi, and which, from the duty it performs, may be denominated the *protective* epithelium, and that which lines the follicles, and is endowed with secreting functions. The same action, then, which, in removing the epithelia from the villi, prepares the latter for their peculiar function of absorption, throws out the secreting epithelia from the follicles, and thus conduces towards the performance of the function of these follicles.

The villi, being now turgid with blood, erected, and naked, are covered or coated by the whitish-grey matter already described. This matter consists of chyme which has undergone the changes induced in it by the bile, of cast off epithelia of the villi, and of the secreting epithelia of the follicles. The function of the villi now commences. The minute vesicles which are interspersed among the terminal loops of the lacteals of the villus (fig. 6, Pl. I.), increase in size by drawing materials from the liquor sanguines, through the coats of the capillaries, which ramify at this spot in great abundance. While this increa[illegible] their capacity is in progress, the growing vesicles are continually exerting their absorbing function, and draw into their cavities that portion of the chyme in the gut necessary to supply materials for the chyle. When the vesicles respectively attain in succession their specific size, they burst or dissolve, their contents being cast into the tissue of the villus, as in the case of any other species of interstitial cell.

The looped network of lacteals, like the other lymphatics, continually exerting their peculiar function, take up the remains, and the contents of the dissolved chyle cells, as well as the other matters which have already subserved the nutrition of the villus. As long as the cavity of the gut contains chyme, the vesicles of the terminal extremity of the villi continue to develope, to absorb chyle, and to burst, and their remains and contents to be removed by the interstitial absorbent action of the lacteals.

When the gut contains no more chyme, the flow of blood to the mucous membrane diminishes, the development of new vesicles ceases, the lacteals empty themselves, and the villi become flaccid.

The function of the villi now ceases till they are again roused into action by another flow of chyme along the gut.

During the intervals of absorption, it becomes necessary to protect the delicate villi from the matters contained in the bowel. They had thrown off their protective epithelium when required to perform their functions, just as the stomach had done to afford gastric juice, and the intestinal follicles to supply their peculiar secretions. In the intervals of digestion, the epithelium is rapidly reproduced. Repeated examinations have induced me to believe that this reproduction is accomplished in the following manner.

That peculiar diaphanous membrane which I have denominated (Trans. Roy. Soc. Ed., 1842) the primary membrane, and which, as I have stated, not only forms the outer membrane of the follicles, under the epithelia, but also the underlying membrane of the villi, contains in its substances nuclei of an oval form, situated at pretty regular distances. These nuclei have a dark spot in the centre, and are always visible when the epithelium is removed. The membrane consists of flattened cells, the nuclei of which continue active. Blood vessels, *therefore, do not* exist in this membrane, but ramify beneath it, as in glands. Serous membranes have a similar constitution.

These nuclei I have elsewhere shewn to be germinal spots or centres of reproduction and growth in the secreting glands (*loc. cit.*). More extended observation has convinced me that they are the centres from which all epithelium, whether secreting or protective, is formed. The process is similar to that described by Reichart and Dr M. Barry, as taking place in the ovum. Cells form in the centre of the spot. These cells increasing in size, and having other cells in their interior, pass off in a radiating direction in the plane of the primary membrane, and gradually assuming the form and properties of the epithelium of the region, till they meet and form a continuous layer of nucleated particles which cover the primary membrane, from whose nuclei they sprung. These nuclei still remain as sources of future crops of epithelia.

During this process of development, the primary membrane would appear to split into two laminæ, the epithelia passing out from its nuclei between these. This would account for the epithelia, particularly the prismatic and conical, adhering by their free extremities.

Such are the processes which would appear to take place in the villi of the intestinal tube during digestion and absorption. When considered in relation to the functions of digestion and absorption of chyle, these processes are highly interesting.

The labours of the chemist have now so far simplified the theory of digestion, as to deprive the stomach of their vitalizing or organizing powers so long ascribed to it.

Every step in this chemico-physiological inquiry leads to the conclusion that the changes which the food undergoes while in the cavity of the gut are entirely of a chemical nature.

If we continue, then, to apply the term digestion to that series of processes by which the aliment is assimilated to the matter of which the body is composed, we must divide the series into two groups. The first group will include all those changes which take place *within* the digestive tube, but *exterior* to the organism. The second will include those which present themselves after the alimentary matter is taken up into *the animal body*, and becomes buried in its substance. The first group of processes are mechanical and chemical in their nature. They may be considered in a great measure as peculiar to the animal, although even vegetables throw out from their roots matter which, acting on some of the materials of the surrounding soil, prepare these for absorption.

The second group of processes is common to animals and vegetables. In these, for the first time, are alimentary substances taken into the tissues of the organism. In animals, as in plants, as I have already pointed out, these alimentary substances are drawn by a peculiar force into the interior of cells, after escaping from which they are taken up by the absorbent system. The chemist has not yet informed us of the change which the matter has undergone during its passage from the cavity of the gut, or from the soil, into the afferent lacteals and the sap-vessels; but if in vegetables, as in animals, this matter passes through the coats and is lodged in the cavities of the cells of the spongiole before it passes on to the sap-vessels, then it is highly probable that the organizing and vitalizing part of the function of digestion commences in the cells of the spongiole and of the extremity of the villus.

The extremity of the fibril of the root of a plant elongates by the cells added to its tissue by the germinating spongiole. The spongiole is, therefore, an active organ of growth as well as of absorption. It is to the fibril of the root what I have denominated in the animal tissues the germinal spot. I conceive it to be probable, therefore, although as to this I have made no observations, that absorption by, and elongation of, the fibril of the root vary inversely as one another. This supposition is founded on the assumption that the cells of the spongiole do not absorb by transmission but by growth and solution.

In the villi of the intestines of animals my own observations lead me to believe that absorption by growth and solution is the process which actually takes place.

The vesicular extremity, like the spongiole of the root fibril, is the primitive germinal spot of the villus. The villus originates in a cell, one of those which form the last deposit from the substance of the yelk. During the development of the villus, this spot or cell was employed only in procuring materials for the growth of the organ. In the perfect animal the formative function of the spot ceases; its action becomes periodical, active during digestion, at rest during the intervals of that process. The same function is performed, the same force is in action, and the same organ, the cell, is provided for absorption of alimentary matters in the embryo, and in the adult, in the plant, and in the animal. The spongioles of the root, the vesicles of the villus, the last layer of cells on the internal membrane of the included yelk, or the cells which cover the vasa lutea of the dependent yelk, and as I have satisfied myself, the cells which cover the tufts of the placenta, are the parts of the organism in which the alimentary matters first form a part of that organism, and undergo the first steps of the organizing process.

### *Explanation of the Plate.*

Fig. 1. Protective epithelium cells from a villus in the dog.

Fig. 2. A group of the same cells adhering by their distal extremities.

Fig. 3. Protective epithelium cells, cast off preparatory to absorption of chyle; instead of nuclei, they present, in their interior, groups of globules.

Fig. 4. Secreting cells thrown out of the follicles of Leiberkuhn during digestion.

Fig. 5. Extremity of a villus immediately before absorption of chyle has commenced. It has cast off its protective epithelium, and displays, when compressed, a net-work of peripheral lacteals. The granular germs of the absorbing vesicles, as yet undeveloped, are seen under its primary membrane.

Fig. 6. Extremity of a villus, with its absorbent vesicles distended with chyle, and the trunks of its lacteals seen through its coats.

Fig. 7. Diagram of mucous membrane of jejunum when absorption is not going on. *a* Protective epithelium of a villus. Secreting epithelium of a follicle. *c c c* Primary membrane, with its germinal spots or nuclei, *d d.* *e* Germs of absorbent vesicles. *f* Vessels, and lacteals of villus.

Fig. 8. Diagram of mucous membrane during digestion and absorption of chyle. *a* A villus, turgid, erect; its protective epithelia cast off from its free extremity; its absorbent vesicles, its lacteals, and blood-vessels turgid. *b* A follicle discharging its secreting epithelia.

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## *On a New Genus, and on Six New Species of Crustacea, with Observations on the development of the Egg, and on the metamorphoses of Caligus, Carcinus, and Pagurus.* By HENRY D. S. GOODSIR, Esq., Surgeon, Anstruther. Communicated by the Author.

### SECTION I.

### *On a new Genus, with descriptions of Three New Species of Stomapoda.*

e first species belongs to the genus Cynthia of Thomson.

A single specimen was obtained in the Frith of Forth, near Anstruther; it was found in shallow water, together with a number of other Schizopoda, and, as far as I am aware, is the first instance of a species of this genus got on the British coasts.

### GENUS CYNTHIA, fig. 1., pl. II.

Subabdominal fins composed of two joints, four last fins with the terminal plume double, with an opaque, bifurcate, and convolute organ raising between each.

C. Flemingii.—Inferior antennal scale almost twice as long as the peduncle. A thick fringe of strong hairs bordering its inner edge. Rostrum slender and finely pointed. Volute organ between the plumose setæ of the subabdominal fins minute; edges of the middle plate of the tail spined.

Long. eight lines. Hab. Frith of Forth.

*Description.*—The whole body of an opaque straw colour, with the reticulated portions of the eyes black. Superior antennæ with the peduncle three-jointed, the two setaceous portions arising from the second joint of the peduncle, the last joint ovate, surrounded with a thick fringe of hairs, these hairs are bent downwards at their extremities, so as to form a concavity on the lower surface. The peduncle is about twice the length of the eyes. The peduncle of the inferior antennæ extends to the origin of the setaceous portion of the superior antennæ, the two last joints are slender and clavate. A long slender and pointed scale arises from the first joint of the peduncle, above the setaceous portion; this is twice as long as the peduncle, and is thickly fringed with long hairs, which are directed inwardly so as to meet those of the opposite side. The carapace is not very large, curved at its posterior edge, and produced at its posterior and inferior angle.

Abdomen slender, the inferior edge of each segment considerably produced, and all of them but the last bearing a fin composed of two joints; the first joint is scale-like clavate; the second is multiarticulate and plumose, all of them but the first pair double. The bifurcate convolute organ between the double plumes is very minute. Middle plate of the tail edged with spines on its sides, and entire at the extremity. External caudal-fins twice as long as the middle plate, and curved upwards, the internal one about the same length as the middle plate, and pointed.

The bifurcate and convolute organ between the double plumes of the four last subabdominal fins, together with the number of joints in these fins, seem to be the most striking characters of this genus. Mr Thompson, in the third memoir of his Zoological Researches, says, "It is not in the *number* of joints alone, however, that they (subabdominal fins) differ, their form and structure is also essentially different. In Cynthia the four last of these members are each composed of a very large bilobate scale, supporting at its apex two taper articulate fins, strongly ciliated with plumose setæ; from between these originates an opaque organ which bifurcates, its two extremes of unequal length being rolled inwards, the one over the other." Mr Edwards considers that these last are the branchial apparatus.

The two animals which are presently to be described, present characters of such a nature as to require the formation of a new genus for their reception. This will constitute a connecting link between the true Opossum Shrimp and the genus Cynthia of Thompson. The subabdominal fins of the genus Mysis, are composed of one short joint, bearing a plumose fringe. The subabdominal fins of the two animals under consideration are of a mixed character, the two first and the fifth are like those of Mysis, the third and fourth are like those of the Cynthia in so far, that the plumes are double, while at the same time these two fins are triarticulate. Thus approaching to the genus Noctiluca of Thompson.

### GENUS *Themisto.*

*Generic Characters.*—Superior antennæ, armed with a scale. First, second, and fifth segments of the abdomen bearing fins like the Mysis; third and fourth with the peduncles, biarticulate, and each peduncle giving off two branches; the external branch of the fourth very long and slender, semi articulated.

*Fig.* 4., pl. II.

*T. Longispinosa.*—Superior antennal scale of the same length, as the terminal joint of its peduncle, armed at its extremity with a thick tuft of hairs. Inferior antennal scale twice as long as its peduncle; fringe not strong. Third subabdominal fin, with its internal branch, minute. Internal branch of the fourth, with a few long hairs from the extremity only. External branch reaching to the extremity of the caudal fins. Internal caudal fin truncated.

*Long,* $\frac{3}{4}$ of an inch.—*Hab.,* Frith of Forth.

*Description.*—The whole body of a dark-yellowish or greenish colour. Eyes large, reaching to the extremity of the peduncle of the inferior antennæ. The reticulated portion black, and produced backwards inferiorly. Rostrum very short, but sharply pointed. First joint of the peduncle of the inferior antennæ very strong, the two following slender; the setaceous portion of the antennæ arising from the extremity last. The scale arises from the inner and superior part of the first joint of the peduncle ; it is hardly twice the length of the peduncle, slender, and tapering very gradually to the extremity ; it is rather thinly fringed. The upper surface of the peduncles of the superior antennæ hollowed out, forming a bed for the eyes. A short ovate scale arises from the inferior part of the last joint, immediately below the origins of the setaceous portions of the antennæ. A thick bunch of matted hair arises from its extremity, which gives it the appearance of being biarticulated. The inferior edge of the external seta of the superior antennæ bears a thin fringe of very strong hairs, which are thickest and strongest near the base. The carapace is not large, leaving two of the thoracic segments exposed posteriorly ; it is rounded at its anterior and inferior angle, and considerably produced at its inferior and posterior angle. A strong biarticulate and chelate palpus arises from each side of the mouth. The abdomen is slender, but the segments are not produced inferiorly. The branchial subabdominal fins are five in number ; they arise from the inferior and posterior angle of all the abdominal segments except the last. The first, second, and fifth are like those in the genus *mysis*, viz., a single plumose joint ; the third and fourth are pedunculated—the peduncles being composed of two joints. The first joint is minute, the second is of considerable length ; two branches arise from the extremity of the second joint ; these branches, in the third fin, are both plumose ; in the fourth one, the internal only is plumose. The external branch of the fourth consists of a very long six-jointed spine, which reaches beyond the extremity of the caudal fins ; it is very finely pointed ; the internal branch about the same as the first joint of the external branch. The caudal plate is slightly swollen near the base ; its edges are serrated, and its extremity bifurcated ; the bottom of the fork being rounded, and the extremities of the fork also blunted and rounded. The internal caudal fins are truncated at their extremities ; the external are paddle-shaped, and rounded at their extremities. Both of these fins are fringed at their extremities, and inferior edges with long hairs.

Fig. 9. Pl. II.

*T. Brevi spinosa.*—Superior antennal scale not so long as the peduncle ; inferior antennal scale four or five times as long as the peduncle. Internal branch of the third subabdominal fin minute ; the internal branch of the fourth longer than the first joint of the external branch ; the external branch extending a little beyond the base of the caudal fins, ending by means of a dart-like point. The lateral caudal fin ending in a sharp point superiorly, and rounded inferiorly ; the internal fin oblong, ovate, and pointed. The lateral edges of the middle plate bearing a single row of long, sharp, and bent spines ; contracted near the base and the bottom of the fork, forming an acute angle ; prongs pointed.

*Long.*, 1 inch.—*Hab.*, Frith of Forth.

*Description.*—The whole body more robust than that of the last described species, and of an opaque white colour, with a single row of black spots along the dorsal mesial line of the abdominal segments. The first joint of the peduncle of the inferior antennæ very short and almost circular; the two following are slender. The scale which arises from the superior part of the first joint above the true antennæ is very strong at the base, and then tapers gradually to a fine point. A fringe of long hairs borders its inferior edge. These hairs are matted at the extremity so as to give them the appearance of a second joint; two or three short strong spines arise from the extremity of the scale. The third joint of the peduncle of the superior antennæ is considerably produced at its superior angle. The scale which arises beneath the setaceous portions is strong, bent upwards at its extremity, and pointed, but it is not fringed. The eyes are large; the reticulated portion circular. The rostrum is of considerable length, but it is not sharp. The internal branch of the third subabdominal fin is minute; the external one is long, slender, and finely pointed; it is also fringed with very long hairs. The internal branch of the fourth fin is longer than the first joint of the external branch; and it is both more strongly fringed and more moveable than that of the last described species. The external branch extends a little beyond the base of the caudal fins. The sixth, or last joint of this branch, suddenly contracts near the extremity to about half its original thickness, ending in a dart-like point. The external caudal fins end in a sharp point superiorly, and are rounded inferiorly; the internal fins are oblong, oval, and pointed at the extremity. These are both fringed at their inferior edges and at their extremities. The lateral edges of the middle plate armed with a single row of strong hooked spines. It is contracted near the base, and the angle formed by the bifurcation is very acute; the extremities of the prongs are also sharp pointed and of a black colour.

## SECTION II.

### *On the developement of the Ova, and on the metamorphoses of Caligus.*

The larva of *Caligus* bears a great resemblance in its external appearance to the larva of *Cyclops* on the one hand, and that of *Lernea* on the other.

As it is my intention to describe how the ovum escapes from the oviduct, it will be necessary, to understand this properly, to give, in the first place, a short description of the organs of reproduction in the female *Caligus.*

These consist of the ovaries, internal and external oviducts, and the vulva through which the oviducts pass. The ovaries are two oval, club-shaped bodies of considerable size, situated on each side, and rather anterior to the stomach. They are large and rounded anteriorly; small, pointed, and converging posteriorly. Each oviduct arises from the external margin of the ovary. It may be divided into three parts, the thoracic, abdominal, and external. The first, or thoracic portion, is very slender; at the posterior part of the thorax, however, it becomes considerably thicker; and, in the abdomen, where it makes four or five convolutions, it is at its greatest diameter; it is considerably contracted as it passes out by the vulva. The external portion is all of the same diameter; and, when arrived at a state of maturity, it is sometimes longer than the body of the animal. But the length altogether depends on the species.

The thoracic portion is colourless; and there a number of small defined objects are seen in it at regular intervals. When it reaches the abdomen these bodies are found to be the immature ova; at this point it is of a delicate carmine colour, and has a striated appearance, from the eggs being placed close together.

The external oviducts, a short time after the ova have escaped, drop off, and new ones soon begin to make their appearance. At first the point only is seen jutting out from the vulva, they then gradually increase until they arive at a state of maturity.

I have not ascertained the length of time which is required before they attain their full growth. Seeing, however, that the distal extremity of the external oviduct is thus first protruded, it may be apprehended that the ova contained in that part of the oviduct are first ready for exclusion, and this accordingly we find to be the case.

The internal structure of the external part of the oviduct, consists of a number of cells placed in a single row, and of the same diameter as the duct; these cells are formed by a number of strong membranous septa, placed at regular intervals. The membranous septa, which divide the oviducts into compartments, are of use, in so far as from their strength they prevent the ovum escaping into the empty part of the duct. They act in the following way :—The ova, as they increase in size, cause a considerable degree of pressure on the membranous septa, and especially on the septum, separating the last ovum fom the empty part of the cell; this by its resistance throws the pressure on the circumference of the cell, this being the weakest part, and the ovum at the same time extending longitudinally, the smaller end of the egg is thrust through the coats of the duct, and thus becomes free. In each of the cells just mentioned, there is an egg, so that the oviduct consists of one row of eggs only.

Thus, as the ova increase in size, the membranes of the oviducts become tense, and burst, and the ovum escapes, but still remains attached to the oviduct, by means of the ovisac, which acts as a cord; the ovum remaining attached to the oviduct by this means, until the young animal is ready to burst from the egg. After the young animal has escaped from the ovum, it remains for a considerable time attached to, or near, the parent animal. The ova belonging to the distal half of the oviduct are generally freed from their attachment to the mother altogether, before those in the proximal half are ready for exclusion from the oviduct. They burst, with very few exceptions, from the external edge of the oviduct; and I have never seen them burst through the membranous septa. They are of a flat shape while in the oviduct, bearing considerable resemblance to a double convex lens; after they have escaped from the oviduct, they become more spherical, and when placed under a powerful glass, the young animal is distinctly seen through the transparent membranes. The young animal at this stage is of a brown colour, with various streaks of purple; the body is of a conical shape, and a constriction or neck separates it from the head. There is no appearance of antennæ, but there are one pair of feet on each side of the body, which divide at their extremities into several long slender spines.

When examined during this stage, under a powerful glass, by means of transmitted light, nothing is perceptible but cellular structure in different degrees of density. The intestinal canal is also perceptible. When the ovum is a little more d, there is sometimes, although not always, the appearance of two small segments arising from the posterior part of the body. The larva, a short time after being hatched, becomes considerably elongated ; it has got three pair of legs, each bearing long spines at the extremity. The third pair were making their appearance before the animal had left the ovum. There are also two spines on each side of the body, at its posterior extremity. The difficulty of keeping these animals alive for any length of time in confinement, has prevented me tracing their further development.

## SECTION III.

### *On Zoe—The development of the Ovum, and the metamorphoses of Carcinus Mœnas, and Pagurus Bernhardus.*

The absence of a metamorphosis in the Crustacea, was considered by Leach to be one of the most decisive characters of the class. This opinion, however, has been overturned by J. V. Thompson, who, by a series of observations, has discovered that the Crustacea do undergo a metamorphosis, and that what have hitherto been considered as perfect animals, viz.—the Zoe of authors, are only the larva of different species of Crustacea Decapoda. Mr Westwood again has asserted, that the Crustacea do not undergo any metamorphosis, bringing forward at the same time observations, the results of which were opposed to those of Mr Thompson. Being anxious to observe for myself, I obtained the different species of the higher Crustacea, common on the shores in this neighbourhood. When loaded with spawn, they were kept alive in vessels of salt water, and in every case I have found the ova to produce Zoe ; and even all the lower Crustacea which I have yet examined undergo a metamorphosis, although not so decided as that of the higher Crustacea. At present I shall describe the Zoe of two of our most common species, viz.—*Carcinus Mœnas* and *Pagurus Bernhardus.*

When the larvæ of the common crab are newly hatched, the body is destitute of spines, but, in a short time, both the dorsal and frontal spines make their appearance (fig. 18, Pl. III.) The eyes at this time are very large, the central or reticulated portion black, with a beautiful shade of green.* The frontal spine as long as the carapace, sharp and pointed. The antennæ arise immediately behind the base of this spine, they are biarticulate, with the peduncle large. The palpi arise immediately behind, and the branchia are in visible and constant motion below the carapace at this spot. The legs are four in number, the first pair at this time composed of two parts, an external and an internal. The external, like those of the Cancer Pagurus, are plumose, being armed at the extremities with three or four strong plumose spines; the internal part arises from the extremity of the first joint; it is multiarticulate, and armed at the extremity with a claw; there is no appearance of this division on the last pair of legs. The abdomen is five-jointed, the last joint armed with two long and curved spines, bearing a number of smaller spines on their inner edge near the base. The intestinal canal is observed running along the lower edge of the abdomen, and the heart may be observed at the base of the dorsal spine, beating synchronously with the motions of the branchiæ. If the young animal is freed from the tunics of the ovum a few days before hatching, its appearance is perfectly different (fig. 16, Pl. III.) The antennæ are long and slender. The eyes are small and sessile, and the caudal fork is armed with a number of curious brush-shaped appendages. (Fig. 17. Pl. III).

The Zoe of the Pagurus (fig. 12. Pl. III.) when it escapes from the egg, or a short time after, is perfectly transparent, the thoracic portion of the body is slightly opaque, and the eyes are black. The abdomen, however, is perfectly translucent, and the observer requires to look very attentively before it can be defined. On being excluded from the egg, the young animal is doubled upon itself; the abdominal portion of the body is bent closely under the thoracic portion, and it is kept in this position by means of a very thin sac or membrane.

It very soon frees itself from this by a few violent efforts, and then the antennæ, the feet, and the abdomen, all become free and extended. The proximate half of the abdomen only is confined within the sac, the distal half is quite free. The Zoe of this species is destitute of spines, the spine on the dorsum of the carapace and the frontal spine being absent. As soon as the young animal frees itself of the sac before mentioned, the thorax apparently becomes much smaller; this arises from the contents of the sac escaping, and the thorax proper only being left. The head is separated from the body by means of a slight constriction. The eyes are very large, reticulated, and black; they are situated on the dorsum and anterior part of the head, on two round tubercles (fig. 15, Pl. III.) there being a notch on the mesial line. These tubercles, I have no doubt, form the ocular footstalks, when the animal has come to maturity. The carapace is about twice the length of the head, its free or lower edge falls considerably down, concealing the origins of the legs; at this part the thorax is quite transparent, the extreme edge being only of a red colour. Immediately below the eyes are the four antennæ; the inferior pair arise from the lower surface of the body, and the first articulation, or what may be termed the peduncle, just reaches the anterior edge of the carapace; it then gives off two articulated setæ, which are considerably longer, very transparent, and taper to exceedingly fine points. The superior antennæ, which are also the innermost pair, are more robust, the peduncle is longer than that of the inferior one; this pair of antennæ act much in the same way as the inferior, only the setæ arises from the superior edge of the peduncle. Four legs arise from the posterior part of the thorax; they are bifid to the first like the antennæ. The abdomen is seven-jonted, about three times as long as the thorax, the last joint spoon-shaped, the lower surface being concave. The posterior edge is armed with a single fringe of strong spines, ten in number; it is notched in the centre. (Fig. 14, Plate II.)

This discovery of a metamorphosis will enable us to decide with greater precision on the species of crustacea, the larvæ of these animals having specific distinctions as decided, doubtless, as those of the insecta.

## SECTION IV.

### *On the Structure and Habits of the Caprellæ; with descriptions of some new Species.*

The genus *Caprella* of Lamarck was separated by Latreille from the Isopoda, with a number of other crustaceous animals, and formed into a distinct order, viz. the *Læmodipoda* or *Læmipoda.* This order has been further subdivided into two families, viz. the *Læmodipoda Filiformia,* and the *Læmodipoda Ovalia.* It is the first or *Filiform Læmodipoda* and the genus *Caprella* of this family which will engage our attention at present. I have had considerable opportunities of examining the animals belonging to this genus in a living state, and by this means have been enabled to draw up a short account of their history. Little is at present known either of their internal structure or habits. This is owing, in a great measure, to two causes; firstly, to their pelagic habitats, and, secondly, to the peculiar structure of their bodies, which much resembles in its external appearance the coralines which they inhabit, on which account they are apt to be overlooked. In general, too, they are almost colourless, although they are very often found of various colours, carmine, green, brown, and all the intermediate hues.

The body of the *Caprellæ*, exclusive of the obsolete abdomen, is composed of seven segments, bearing five pairs of ambulatory legs; the second and third thoracic segments, each bearing one pair of membranous finlets, which act, to a certain extent, as organs of respiration. There are two pairs of antenna, and the mechanism of the mouth* is rather complicated. The digestive canal is a simple straight canal, which, during its course through the post occipital and first thoracic segments is considerably dilated; after this, however, it runs to the distal extremity of the body without farther complication. It is seen to pulsate at irregular intervals, and this is observed at that part only immediately above the ovarian openings; the ovaries when present, are attached to the digestive canal at this point by cellular structure, and by this means they also are subject to this pulsation. The vascular system in the caprellæ is composed of two sets of vessels, the one arterial, the other venous. These are most easily observed in the antennæ; the main artery runs along the superior, and the vein along the inferior edge of the antenna; lesser branches either spring from or run into these main trunks. A large dorsal vessel is seen in the trunk; but owing to the greater opacity of the body, I have been unable to trace the anatomy of the blood-vessels farther. On placing a live caprella under the microscope, the blood is seen circulating through the vessels of the antennæ in a most beautiful manner, passing from the body by means of the superior or arterial trunk, and returning again by means of the inferior or venous. The blood globules in these vessels are seen passing onwards steadily and continuously; some of them are often lost to the field of the microscope, passing off probably by means of the smaller lateral branches. Sometimes it is oscillatory, that is to say, the globules are seen vibrating backwards and forwards, but this is only occasional, in general it is continuously onwards, frequently, however, they jerk suddenly forwards, but they subside as rapidly into their usual course. The circulation does not seem to be guided by any apparent pulsation; the globules are spherical, not very numerous, and are suspended in a colourless liquid. I have not been able as yet to detect any appearance of a nervous system.

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* M. Edward's Hist. des Crust., vol. iii. p. 103.

The organs of reproduction are extremely curious, and are complicated in the female. I am not acquainted with their peculiar organization in the male.

The ovaries (fig. 2. Pl. III.) are two long slender bodies which lie along each side of the intestinal canal. Each of them arises from the posterior part of the post-occipital segment; during its course through the first thoracic segment it is thin and slender, but as it reaches the second it becomes considerably larger: and about the middle of this segment each of them gives off a small duct—the oviduct; these meet one another in the mesial line by means of one common opening through the roof of the ovarian pouch; another duct is given off in the same way at the second ovarian opening, and the ovaries then end at the posterior extremity of the fourth thoracic segment. The internal structure of the ovaries is like those of the caligi; they consist of a single row of cells, each of which contains an egg. The external organs of generation in the female are composed of a series of plates four in number, which, when enlarged, forms a floor to a corresponding cavity in the abdominal surface of the two middle thoracic segments; this forms the oviferous sac or pouch. When the animal is with spawn, these two thoracic segments become more intimately joined together, that is to say, their common joint becomes a fixed suture; the two ovarian cavities also approximate on their lower surfaces, and become opposed to one another, thus allowing the ovarian plates to overlap one another, and cover the eggs without becoming inconveniently large.

The ovarian plates (fig. 3. Pl. III.) as I have stated formerly, are four in number; they are of an irregular oval form, and are armed on their edges with a single row of strong hooked spines; these seem to be of use in so far as to prevent the over-distension of the plates by means of hooking into one another. The ova are not large, quite globular, and of an opaque yellow colour. On one occasion, while examining a female *Caprella* under the microscope, I found that her body was thickly covered with young ones; they were firmly attached to her by means of their posterior feet, and they were resting in an erect posture, waving about their long antennæ with great activity. The *Caprellæ*, like all the lower crustacea, cast their skins often. Before the process commences, the animal lies for a considerable time languid, and to all appearance dead; at length a slight quivering takes place all over the body, attended in a short time with more violent exertions; the skin then bursts behind the head in a transverse direction, and also down the mesial line of the abdominal surface; a few more violent exertions then frees the body of its old covering. After this the animal remains for a considerable time in a languid state, and is quite transparent and colourless.

The habits of the *Caprellæ*, arising from the difficulty of keeping them alive for any length of time, are little known. They are in general local in their habitats, frequenting coralines which are found in deep water. They are never seen catching prey; slow and deliberate in their motions, they are not fitted for this mode of life. While at rest, they firmly adhere to the object by means of their posterior thoracic legs, generally in an erect posture, waving about their long antennæ, by which means they bring within their reach animalcules and other objects of food, which are then drawn into the mouth. Their usual mode of progression is like that of the larvæ of the Geometer moths, as mentioned by Montague. They sometimes walk in this way for a considerable time, and then suddenly stop, remaining perfectly motionless, not even moving their antennæ. I have never seen them swim, and even when pushed off their resting-place, they fall listlessly to the bottom of the vessel in which they are confined.

## *Caprella.* Lamarck.

*Generic characters.*—Branchial lamellæ, two pairs attached to the second and third segments, which are apodal.*

### BODY ARMED WITH SPINES.

Plate III. Fig. 1.

*Caprella spinosa.*—One spine arises from the dorsum of the head, another from the posterior extremity of the post-occipital segment, other two arise in the same transverse line on the dorsum of the swellings of the first thoracic segment, and a fifth from the posterior extremity of this segment. Superior antennæ as long, or almost as long, as the body, last joint multiarticulate. Swelling on the first thoracic segment near the anterior extremity. Two spines on the lower edge of the last joint of the second pair of feet. Long, 1 inch; hab., Frith of Forth.

*Description.*—The whole body of a pale white colour, with the exception of the eyes, which are black. The dorsal surfaces of the two first segments of the body are armed with long sharp curved spines, which point forwards; one arises from the vertex of the head, another from the posterior extremity of the post-occipital segment, other two arise in the same transverse line from the swelling on the first thoracic segment, and a fifth from the distal extremity of the same segment. Superior antennæ as long as the body; first joint obsolete; second and fourth as long as the head and post-occipital segment conjoined; third almost twice as long; fifth multiarticulate, and as long as all the others conjoined; it is composed of about 26 articulations; fourth articulation slightly clavate. Inferior antennæ a little longer than the three first joints of the superior antennæ; the two first joints equal in length to the palpi, the third reaching a little beyond the second joint of the superior pair, fourth and fifth reaching beyond the third, fifth triarticulate.

Head rounded considerably, produced inferiorly, eyes circular, black, and reticulated; post-occipital segment twice as long as the head; first thoracic segment with the swelling before the middle, and a spine pointing forwards immediately above the origin of the second pair of legs; femoral joint clavate, twice the length of the first thoracic segment; last

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* Dr Johnston, 8th vol. of Mag. Nat. Hist. p. 669.

joint oblong ovate, produced into a small blunt tooth at its anterior edge; lower edge armed with two strong spines; the interspace armed with a number of strong spines; claw strong; apodal thoracic segments considerably lengthened, slightly contracted posteriorly. The origin of the branchial lamellæ placed behind the middle of the segment. Posterior throacic segments nonpediculated; legs long, bearing a few bristles; inferior edge of the last articulation of the last pair of legs armed near the base with a small tooth.

This species differs from the *Caprella Phasma* of Colonel Montague in having five spines on the first thoracic segment, and from the segments of the body being considerably longer. The third joint of the superior antennæ is very much longer, and the first pair of feet are also minute and slender, differing in so far from those of *Phasma*, which are strong and powerful. The inferior edge of the last joint of the second pair of feet is also armed with two strong spines, whereas in the *Phasma* there is only one strong spine.

Plate III. Fig. 6.

2. *Caprella tuberculata.*—Body robust. Superior antennæ almost reaching to the first finlet; fourth joint about half the length of the third; fifth joint composed of about thirteen articulations. A short spine on the summit of the head almost as long as the post-occipital segment; swelling of the first thoracic segment exactly in the middle; a short tubercle on the summit. Femoral joint of the same length as the depth of the segment; last joint oval. Four tubercles placed in a square form on the dorsum of the fourth thoracic segment. A spine on the last joint of all the three last legs. Long. ¾ of an inch. Hab. Frith of Forth.

*Description.*—Whole body of an opaque yellow colour. All the joints of the body bearing spines or tubercles on the dorsal surface. Head nearly spherical, with a short sharp spine on its vertex, almost as long as the post-occipital segment. Eye small and spherical. Superior antennæ reaching to the middle of the second thoracic segment; first joint obsolete; second as long as the head and post-occipital segment conjoined; third longer; fourth joint about half as long as the third; fifth multiarticulate, moniliform, and composed of fourteen articulations armed with spines on their lower edge. Inferior antennæ reaching to the middle of the fifth joint of the superior antennæ; two first joints together equal in length to the half of the third, fourth, and fifth of the same length; last joint armed with a claw. Eyes small, black. Post-occipital segment a very little longer than the head, and bearing two blunt tubercles at each extremity. First thoracic segment twice as long, swelling very little, and situated in the middle; a blunt short tubercle surmounts its dorsal surface. The femoral joint is very short and robust—it is angular, that is to say, bearing three ridges; last joint is large and oval, serrated on its inferior edge; a large tubercle is situated near the base; the claw is short but powerful. The second and third thoracic segments are of the same length, second more slender: branchial lamellæ are short, and arise considerably behind the middle, and a blunt tubercle arises from the dorsum of the segment immediately above the origin of each. Fourth joint is very rough—it is armed with a tubercle on its dorsal surface at its anterior edge; another larger and flatter one immediately below this on its lateral surface, and four other sharper ones placed in the figure of a square at its posterior part. The two following joints are pediculated, and bearing two sharp spines on each. All the joints of the posterior thoracic legs are powerfully clavate, which gives them a serrated appearance; a short strong tooth arises from the inferior edge of the last joint near the base.

This species is apt to be confounded with the *Caprella acanthifera* of Leach, but may be distinguished from it by the double fringe of spines on the lower edge of the inferior antennæ; the superior antennæ are also much shorter than those of the *acanthifera*.

### BODY FREE FROM SPINES.

Plate III. Fig. 4.

*Caprella lævis.*—Body without spines. Superior antennæ not reaching the origin of the second pair of legs; last joint composed of six articulations. Head almost triangular; post-occipital segment four times as long as the head. Swelling on the first thoracic segment near the posterior edge; coxal joint large produced. Length, one inch.

*Description.*—The whole body is quite smooth, shining, and free of spines. The head is almost triangular. Eyes near the upper surface and very small. The superior antennæ are very short and strong: third joint very strong, and as long as the two following: last joint composed of thirteen articulations. Inferior antennæ not so long: the two first joints very short: inferior edge of all the articulations armed with a strong fringe of double spines. Front pair of legs strong, and almost as long as the lower antennæ.

Post occipital segment slender, and four times as long as the head. First thoracic segment very long, slender at its anterior extremity, and with the swelling entirely situated at its posterior extremity. Second pair of legs arises from the anterior part of the swellings: coxal joint large and produced: femoral joint considerably bent and slightly clavate: last joint oblong ovate and armed on its inferior edge with two spines, one near, or rather at the middle, which is smallest, and the other about its own length from the anterior extremity: a number of denticles and strong spines arm the interspace: claw strong and bent: slightly flattened on its superior edge.

Second and third thoracic segments very short, wanting about a third of the length of the first thoracic: fourth segment equal in length to either of the two former: fifth and sixth pediculated, last almost globose. Segments of all the posterior legs lengthened; last joint of the last pair with a tooth on its inferior edge near the base: claws strong.

This species may be distinguished from *Caprella linearis*, with which it is most apt to be confounded, by its greater comparative size, the structure of the antennæ; by the shortness of the post-occipital segment; the situation of the swelling on the first thoracic segment, which is at the posterior edge, whereas in the *linearis* it is at the anterior; the femoral joint of the second pair of legs is not clavate in the *linearis*, and is also quite straight.

Plate III. Fig. 8.

*Caprella linearis.*—Superior antennæ reaching to the middle of third thoracic segment: head very deep: post-occipital segment extremely short, almost of the same length as the head. Swelling at the anterior extremity of the first thoracic segment. Femoral joint of the second pair of legs almost as long as the first thoracic segment: last joint oval; one tooth on its lower edge near the base. Length, half an inch. Hab. Frith of Forth.

*Description.*—The whole body almost colourless: very fragile. Head very deep, being almost twice as deep as the post-occipital segment. Superior antennæ reaching almost to the middle of the third thoracic segment. Last joint composed of about twelve articulations. Inferior antennæ hardly so long, and strongly fringed with spines on its lower edge. The post-occipital segment is extremely short, hardly so long as the head itself. First thoracic segment twice as long as the head and post-occipital segment: the swelling is situated near the anterior extremity at this part: it gives off a small branch from which the second pair of legs arises. The femoral joint is not clavate, quite straight, and of no great length, not being so long as the first thoracic segment: last joint oval, almost globular, and armed with one tooth on the inferior edge near its base. Claws small. Second and third thoracic segments of equal length; second more slender. Branchial lamellæ small and situated near the middle of the segment. Fifth and sixth thoracic segments pediculated. The posterior thoracic legs almost moniliform; an almost obsolete tooth being placed on the inferior edge near the base of distal segment of the last pair of feet.

This appears to be the *Caprella linearis* of authors; there e some marks of difference, but they are trivial, and not fficient to authorize any new specific distinctions.

## *Description of Plate II.*

Fig. 1. Cynthia Flemingii.
... 2. Part of inferior antennæ, with scale.
... 3. Natural size of Cynthia Flemingii.
... 4. Themisto longispinosa.
... 5. The third and fourth subabdominal fins; *a*, peduncle of the third; *b*, external branch; *c*, internal branch; *d*, peduncle of the fourth subabdominal fin; *e*, internal branch; *f*, the long external branch.
... 6. The superior antennal scale of T. longispinosa.
... 7. Nat size of T. longispinosa.
... 8. Middle caudal plate.
... 9. T. brevispinosa.
... 10. The third and fourth subabdominal fins of T. brevispinosa; the letters refer to the same parts as those in the same drawing of the T. longispinosa.
... 11. Natural size of T. brevispinosa.
... 12. The middle caudal plate.

## *Description of Plate III.*

Fig. 1. Caprella spinosa.
... 2. Natural size.
... 3. Ovarian plate, and one of the branchial fins attached, it shews the upper or concaved surface with the hooked spines.
... 4. Caprella lævis, magnified.
... 5. Natural size.
... 6. Caprella tuberculata, magnified.
... 7. Natural size.
... 8. Caprella linearis, magnified.
... 9. Natural size.
... 10. Magnified lateral view of the second and third thoracic segments of *Caprella spinosa*, shewing how the segments become changed during spawning, with the ovarian plates and branchial finlets in the natural position.
... 11. Enlarged view of Ovaries A. and Oviducts B.; *a*, anterior extremity; *b*, posterior.
... 12. Zoe of *Pagurus Bernhardus*, lateral view.
.. 13. Back view.
... 15. Ocular tubercles.
... 14. Caudal segment.
... 16. Lateral view of the Zoe of the *Carcinus mænas*, sometime before it has escaped from the ovum. The tunics of the egg have been stripped off.
... 17. Last abdominal segment, and caudal fork of the same, shewing the curious brush-shaped appendages; these drop off when the animal has escaped from the ovum, and are replaced by spines.

Fig. 18. Side view of the Zoe of the *Carcinus mænas*, a short time after it has escaped from the ovum.

... 19. Appearance of the Larva of the *Caligus piscinus*, sometime after it has escaped from the ovum.

... 20. Mature ovum of the same.

... 21. Ovum in an early stage.

... 22. Ovum a little farther advanced.

... 23. Young Caligus taken from the egg; it is at a more advanced stage than the last.

... 24. External oviduct of Caligus, shewing the escape of the ova, and how they remain attached until the young are hatched.

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## *Additional Observations On Fibre, contained in a Memoir lately read to the Royal Society of London.* By Martin Barry, M.D., F.R.S., Lond. and Ed.

On examining coagulating blood, the author finds that it contains discs of two different kinds; the one comparatively pale; the other, very red. It is in the latter discs that a filament is formed; and it is these discs which enter into the formation of the clot; the former, or the pale discs, being merely entangled in the clot, or else remaining in the serum. He thinks that the filament escaped the notice of former observers, from their having directed their attention almost exclusively to the undeveloped discs which remained in the serum, and thus conceived that the blood-discs are of subordinate importance, and are not concerned in the evolution of fibrin.

To render the filament distinctly visible, Dr Barry adds a chemical reagent capable of removing a portion of the red colouring matter, without altogether dissolving the filament. He employs for this purpose chiefly a solution of one part of nitrate of silver in 120 parts of distilled water; and sometimes also the chromic acid. He admits that the use of these reagents would, on account of their destructive tendency when concentrated, be objectionable as proofs of the absence of any visible structure; but as the point to be proved is, that a certain specific structure does exist, he contends that the same appearance would not equally result from the chemical actions of reagents so different as are those of chrome and the salts of mercury and of silver. After the appearance of the filament, thus brought to light, has become familiar to the eye, it may be discerned in the blood-discs, when coagulation has commenced, without any addition whatever. Those blood-discs of the newt, which contain filaments, often assume the form of flask-like vesicles, the membranes of which exhibit folds, converging towards the neck, where, on careful examination, a minute body may be seen protruding. This body is the extremity of the filament in question, its protrusion being occasionally such as to admit of its remarkable structure being recognised.

The author proceeds to describe various appearances which he has observed in the coagulum of the blood, and which strongly resemble those met with in the tissues of the body, and are obviously referable to a similar process of formation. He bears testimony to the accuracy of the delineations of coagulated blood given by Mr Gulliver. One of the most remarkable phenomena discovered by the author in the coagulation of the blood is the evolution of red colouring matter; a change corresponding to that which he had previously observed to take place in the formation of the various structures of the body out of the corpuscles of the blood. He considers the production of filaments as constituting the essential circumstance in coagulation.

He conjectures that the notched or granulated fibres noticed in the blood by Professor Mayer, may have been of the same kind as the flat, grooved, and compound filaments described by himself; but he thinks that, in that case, Mayer's explanation of their mode of origin must be erroneous; for they may be seen to be produced by a portion of the blood not mentioned by him, namely, the corpuscles.

Mr Addison's discovery of globules in the uppermost stratum of inflammatory blood, and of their influence in the formation of the buffy coat, is confirmed by Dr Barry, who remarks that these globules are altered red blood-discs. That the blood corpuscles are reproduced by means of parent cells, as suggested by Mr Owen and by the author, is confirmed by the observations of Dr Remak; but the author had long ago indicated a division of the nucleus as being more particularly the mode of reproduction, not only of those corpuscles, but of cells in general. With this conjecture the observations of Remak on the blood-corpuscles of the fœtal chick fully accord. Whether the author's further speculation, namely, that the parent-cells are altered red blood-discs, is correct, still remains to be seen.

The phenomenon of the "breaking off short," or notching of the fasciculus of a voluntary muscle in a transverse cleavage of the fibre, is regarded by Dr Barry as a natural consequence of the interlacing of the larger spirals, which he has described in a former paper; the fracture, in proceeding directly across the fasciculus, taking the direction in which there is least resistance.

The position of the filament in the blood-corpuscle is represented as bearing a striking resemblance to that of the young in the ovum of certain intestinal worms, the filaments of which are reproduced by spontaneous division. The author subjoins the following quære, "Is the blood-corpuscle to be regarded as an ovum?"

(We are requested by Dr Barry to state that the above abstract contains several errors, among which are the following (see paragraphs 5 and 7.)

1. That the blood-corpuscles are reproduced by means of parent cells, he had not "suggested," but clearly shewn.

2. That a division of the nucleus is more particularly the mode of reproduction, was no "conjecture," but recorded as a fact.

3. That the parent-cells are altered red blood-discs, was also an observed fact, and not a "speculation."

4. It is the filament formed in the blood-corpuscle that Dr Barry describes as being reproduced by self-division, and not the filaments of intestinal worms.—*Edit.*

## *Lord Gray's Meteorological Table for* 1841.

**Extracted from the Register kept at Kinfauns Castle, North Britain.**

**Lat. 56° 23′ 30″.—Above the level of the sea, 150 feet.**

| 1841. | MORNING, ½ past 8. Mean Height of | | EVENING, 8 o'clock. Mean Height of | | Mean Temperature by Six's Thermometer. | | | Depth of Rain in Garden. | No. of Days. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Barom. | Ther. | Barom. | Ther. | Lowest. | Highest. | Mean. | | Rain or Snow. | Fair. |
| January,...... | 29.648 | 29.258 | 29.611 | 30.516 | 25.451 | 36.677 | 31.064 | 2.30 | 10 | 21 |
| February, ... | 29.713 | 36.928 | 29.705 | 37.000 | 34.928 | 42.000 | 37.964 | 1.85 | 13 | 15 |
| March, ...... | 29.584 | 43.580 | 29.590 | 42.667 | 39.064 | 52.484 | 45.774 | 1.55 | 16 | 15 |
| April, ......... | 29.642 | 43.133 | 29.664 | 41.266 | 36.366 | 52.100 | 44.233 | 1.38 | 9 | 21 |
| May,........... | 29.396 | 51.741 | 29.674 | 49.838 | 42.193 | 60.709 | 51.451 | 1.56 | 9 | 22 |
| June, ......... | 29.713 | 53.900 | 29.787 | 51.800 | 44.867 | 61.933 | 53.400 | 1.91 | 7 | 23 |
| July,........... | 29.621 | 56.032 | 29.625 | 53.935 | 49.193 | 63.967 | 56.580 | 3.96 | 15 | 16 |
| August, ...... | 29.627 | 57.225 | 29.656 | 54.000 | 49.774 | 64.096 | 56.935 | 4.76 | 16 | 15 |
| September,.. | 29.597 | 52.066 | 29.595 | 50.933 | 48.432 | 60.700 | 54.566 | 2.65 | 16 | 14 |
| October,...... | 29.430 | 42.832 | 29.425 | 40.742 | 37.418 | 50.000 | 43.709 | 4.66 | 23 | 8 |
| November,... | 29.504 | 35.933 | 29.501 | 36.700 | 32.933 | 41.933 | 37.433 | 2.12 | 12 | 18 |
| December,... | 29.377 | 36.806 | 29.410 | 36.258 | 33.354 | 41.742 | 37.548 | 2.40 | 15 | 16 |
| Average of the year, | 29.563 | 44.953 | 29.595 | 43.805 | 39.414 | 52.362 | 45.888 | 31.10 | 161 | 204 |

## ANNUAL RESULTS.

### MORNING.

| BAROMETER. | *Observations.* | *Wind.* | THERMOMETER. | *Wind.* |
|---|---|---|---|---|
| Highest, | 1st February, | E. 30.56 | 26th May, | SE. 66° |
| Lowest, | 30th November, | SW. 28.43 | 9th January, | NW. 6° |

### EVENING.

| | | | | |
|---|---|---|---|---|
| Highest, | 1st February, | E. 30.55 | 20th August, | E. 65° |
| Lowest, | 29th November, | E. 28.60 | 8th January, | NW. 5° |

| *Weather.* | *Days.* |
|---|---|
| Fair, | 204 |
| Rain or Snow, | 161 |
| | 365 |

| *Wind.* | *Times.* |
|---|---|
| N. and NE. | 38 |
| E. and SE. | 95 |
| S. and SW. | 108 |
| W. and NW. | 124 |
| | 365 |

*Extreme Cold and Heat by Six's Thermometer.*

| | | | |
|---|---|---|---|
| Coldest, | 9th January, | Wind NW. | 3° |
| Hottest, | 20th August, | Wind SE. | 72° |
| Mean Temperature for the year 1841, | | | 45°.888 |

*Results of two Rain-Gauges.*

| | In dec. |
|---|---|
| 1. Centre of Kinfauns Garden, about 20 feet above the level of the sea, | 31.10 |
| 2. Square Tower, Kinfauns Castle, 180 feet, | 30.29 |

Professor Jameson then exhibited finely preserved specimens of the following large fishes, taken lately in the Frith of Forth, and chiefly in Aberlady Bay :—1. A Tunny, 8 feet long, and five feet 6 inches in circumference where thickest ; 2. Portbeagle Shark, 7 feet 3 inches long, by 4 feet in circumference ; 3. Great Sunfish, 5 feet 2 inches in length, and 2 feet 8 inches in depth ; and, 4. Conger Eel, 6 feet 6 inches long, 1 foot 10 inches circumference. The Professor also exhibited a beautifully spotted Seal, 5 feet 6 inches long, 4 feet 3 inches circumference, which had been entangled and drowned in a herring net off Inchgarvey, near Queensferry.

*April* 16.—Professor JAMESON, President, in the Chair.

The assistant Secretary read a memoir, giving a description of a magnetical instrument invented and constructed some years ago by Mark Watt, Esq. of Edinburgh, which, standing upon a table in any room, and secluded under a crystal shade, points in the direction of the wind. This instrument is formed of a thin bar of wood, of three or four inches long, which traverses like a compass, upon a steel pivot, by means of an agate-capsule inserted into the wood. Three or four magnets are affixed to one end of the bar of wood, which has a slit one-third of its length to receive them. They are placed in a line, at a distance of half an inch from each other. The magnets are very light, being pieces of the mainspring of a watch, made straight, of different lengths, increasing from one inch to three. They are fixed quite perpendicular to the horizon, and therefore deprived of polarity, with all the south poles uppermost and north undermost. The instrument is not a perfect vane ; for although the bar of wood stands exactly according to the direction of the wind, it is indifferent to it which end it turns towards the point the wind blows from. Yet several rather interesting deductions in physical science can be drawn from it. It evinces the connexion between magnetism and electricity. It also renders it probable that our variable winds are caused by electrical currents, as this instrument anticipates the changes of the wind by a quarter or sometimes half an hour. The instrument was exhibited and explained by Dr Glover.

Mr John Goodsir read a paper by Mr H. D. S. Goodsir on the metamorphoses of Carcinus and Pagurus, and on some new species of Caprella (see p. 174.)

Dr Robert Hamilton then read a paper, entitled : Report on the more recent investigations concerning the structure, habits, and economic uses of Fishes.

## *List of Prizes of the Royal Scottish Society of Arts for Session* 1842-43.

The ROYAL SCOTTISH SOCIETY OF ARTS proposes to award Honorary Medals, and Pecuniary Prizes, for approved Communications.

*No Prize to exceed Thirty Sovereigns.*

The attention of the Fellows and of the Public is directed to Inventions, Discoveries, and Improvements in the ***Mechanical*** and *Chemical* Arts in general, and also to means by which the ***Natural Productions*** of the Country may be made more available; and in particular to,—

EXPERIMENTS applicable to the Useful Arts.

INVENTIONS, Processes, or Practices from Foreign Countries.

PRACTICAL DETAILS of PUBLIC or other undertakings of National importance,—not previously published.

METHODS of Economising Fuel, Gas, &c.—of preventing Noxious Vapours from Manufactories,—of procuring small, intense, and uniform sources of Heat.

IMPROVEMENTS in Instruments for Measuring minute quantities of Heat,—in the Manufacture of Iron, and other Metals, in the making and Tempering of Steel,—in Calotype, Daguerreotype, and Electrotype,—in the preparation of Lime and Plaster for Fresco Painting, and for appropriate Tools for laying the Plaster with precision,—in Electric,—Voltaic,—and Magnetic apparatus,—in Pavements,—in Balance or Pendulum Time-keepers,—in Taps and Dies,—in Glass and Porcelain,—in Land, Marine, and Locomotive Steam-Engines, and Railway Carriages,—in Railway Telegraphs and Signals, &c., &c.

### KEITH PRIZE.

The SOCIETY will also award the KEITH MEDAL, value THIRTY Sovereigns, for any important Invention, Discovery, or Improvement in the Useful Arts.

*General Observations.*—The KEITH MEDAL may be awarded for any communication which shall comply with the terms of the announcement of that Prize, although falling under any of the above specified subjects.

The descriptions of the various inventions, &c. to be *full* and *distinct*, and, when necessary, accompanied by *Specimens*, *Drawings*, or *Models*.

The Society shall be at liberty to publish in their Transactions copies or abstracts of all Papers submitted to them. All Models, Drawings, &c. for which Prizes shall be given, shall be held to be the property of the Society,—the Society being in the practice of taking the value of the Model into account in fixing the amount of the Prize.

All Communications must be written on *Foolscap* paper, leaving margins at least one inch broad, on *both the outer and inner sides of the page*, so as to allow of their being afterwards bound up with others; and all Drawings must be on *Imperial* Drawing Paper, unless a larger sheet be requisite.

All Communications to be lodged *as soon after* 1*st November* 1842 *as possible*, in order to ensure their being read during the Session; *but those which cannot be lodged so early, will be received till* 1*st March* 1843.

Communications, Models, &c. to be addressed to James Tod, Esq., the Secretary, 21 Dublin Street, Edinburgh, Postage or Carriage paid.

Copies of this List of Prizes may be had from the Secretary.

Edinburgh, 11*th April* 1842.

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## SCIENTIFIC INTELLIGENCE.

### GEOLOGY AND MINERALOGY.

1. *On Fan-Shaped Stratification.*—A distinguished geologist in a letter lately received says,—"It would, without doubt, be making a great step in geology, if we could explain the schistose structure and the stratification of crystalline formations by means of the structure assumed by ice. I believe that we are still far from being even at the commencement as regards all those things which depend on molecular actions. The explanation, for example, given of the columnar structure of basalts, has always appeared to be but little satisfactory; and only leads to a mode of accounting for a division of a somewhat different description. But the structure which has occupied me more than any

other for many years, and which seems to me to contain the key of the whole theory of the crystalline formations, is the fan-shaped structure of the central masses of the Alps. It is first of all necessary to know if these strata are true beds of sediment, originally horizontal, or if the fan-shaped division is secondary and dependent on molecular actions; and I have not yet been able to decide between these two modes of viewing the subject. We are in possession of very favourable arguments for the second. I find one in the great regularity of strata extending for great distances without the least inflexion; whereas the calcareous and schistose formations of the Alps generally exhibit contortions which announce the most violent movements throughout their whole mass. It is but little probable that these movements should have acted only on the edges of the central masses; and one would be almost led to admit, that at the centre the sedimentary structure has been entirely effaced. Another argument results from the direct observation, that near the contact of the limestone and the gneiss, the vertical structure of the gneiss is never visible; that, on the contrary, the gneiss, or rather a mixed rock, follows the stratification of the limestone, and even alternates several times with the beds of limestone which contain organic debris; and that it is only at some distance from the limit of the two rocks that the vertical structure presents itself. This is evident, for example, in the Grindelwald, at the contact of the Mettenberg and the Schreckhorn, and in the valley of Urbach. But other facts which do not appear less decisive, are more favourable to the opinion which regards the fan-shaped strata as true elevated beds. There is first the alternation of various rocks, different varieties of granite, gneiss, amphibolite, &c. in the same fan; and the fact that this structure is not limited to crystalline rocks, but extends at the two extremities of the central crystalline mass, very far into the sedimentary formations which surround it; finally, that, at the two sides of the central mass which are parallel to its direction, the crystalline beds are in concordant stratification with the sedimentary beds, which themselves form part of the fan. I had hoped to work out this grand problem in my last year's tour, but I am now no farther advanced than I was a year ago."

2. *Dr Houghton on the Geognostical position of the numerous masses of native Copper in North America.*—It is a fact well known, that south from the northern peninsula of Michigan, transported masses of native copper are occasionally met with in the diluvial deposits which are so abundantly spread over the country; and these loose masses are distributed over an area of many thousand miles, including Southern Michigan, Wis-

consin, Illinois, and Indiana. In Northern Michigan they are still more frequently met with.

The great transported mass of native copper on the Ontonagan river, so frequently alluded to by travellers, and which he, Dr H. estimated to contain about four tons of native metal, was stated to have all the characters of the other loose masses referred to.

The source of these transported masses has hitherto been somewhat obscure, although there has been good reason to believe, that most of them had their origin from the trap-rocks; but whether from true veins, or from the mass of the rock itself, was not known. He said, that, after examining the country with care, he was enabled to state, that, without doubt, a very considerable portion of them had their origin from what may be regarded as true veins.

Those which were regarded as true veins, were uniformly noticed to originate in the trap-rock, but they were frequently traced across the superimposed sedimentary rocks, including the red sandstone. The direction of the veins across the upper rocks most frequently corresponds to the dip of those rocks.—*American Journ. of Science,* vol. xli. No. i. p. 184.

3. *The Great Crater of the Volcano in Hawaii.**—" It is an immense pit, one thousand feet deep, and six miles in circuit, with perpendicular walls, except at one point, where it is reached by a deep descent, and the whole of this vast cauldron full of boiling, bubbling, and spouting lava. The surface at one moment as black as ink, and the next exhibiting rivers, and pools, and jets of a hideous blood-red fluid, that was sometimes thrown up to a height of fifty or sixty feet, and fell back with a sullen plashing that was indescribably awful. The aspect of the whole was *hellish*—no other term can express it. By night it was grand beyond description. The frequent lightings up, the hissings, and deep muttering explosions reminded me of some great city in flames, where there were magazines of gunpowder or mines continually exploding. Vesuvius is tame in comparison with it. Just previous to my visit the lava had burst out at a new place, about six miles north-east of the crater, and flowed down to the sea in a stream of *forty miles in length* by from *one to seven in breadth.* I saw the light one hundred miles off. It reached the sea in five days; threw up three hills of from one hundred and twenty to two hundred and fifty feet high; gained two thousand feet out seaward from the old line of coast by three-fourths of a mile in width, and heated the water for fifteen miles on either side to such an extent that the fishes were heaped up in myriads on the shore,

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* In a letter to Professor Silliman, dated Honolulu, Oahu, Oct. 24, 1840. —*Silliman's* Journal, vol. xli., p. 200.

scalded to death. Its falling into the sea was accompanied with tremendous hissings, and detonations like constant discharges of heavy artillery, distinctly heard at Hilo, twenty minutes distant.—Yours, &c.

D. H. Storer.

4. *Jamesonite.*—From Las-Parets, (Ann. des Mines 3ème ser. xviii.)—An interesting locality of this mineral has been lately discovered between Milhau and Severac-le-Chateau. The surrounding rock is a yellow, granular, distinctly stratified limestone, abounding in magnesia, and traversed by many veins of heavy spar. The Jamesonite occurs in drusy cavities, either in a pure state or mixed with heavy spar. It afforded, on being analysed, the following ingredients:—viz. lead, 48.8—copper, 6.6—antimony, 17.2—sulphur and loss 27.4 = 100.0.

5. *Crystallized Gold.*—On examining many crystals of gold, from the gold washings of Catharinenberg, Mr Avdeeff obtained the same results as those of G. Rose in his analysis of the rolled masses of gold of the Urals, viz., that the gold in veins and seifenwerken, whether massive or crystallized, is combined with silver in indeterminate proportions, and that both substances are isomeric. According to Avdeeff, the crystals of gold in rhombohedral dodecahedrons contain much more gold than those which occur in tetrahedral and octahedral crystals. Is there a determinate limit to the amount of gold and silver according to which crystals take on this or that form?—*Leonhard and Bronn's Jahrbuch,* 6 *Heft,* 1841.

6. *On the Composition of the Asbestus of Scharzenstein in the Ziller Thal in the Tyrol.*—Meitzendorff has lately given, in Poggendorff's Annals, an analysis of this variety of asbestus, which is characterized by the length of its fibres and by its white colour. 100 parts contain—

| | | Oxygen. | |
|---|---|---|---|
| Silicic acid, . . . . . . | 55.869 | 29.023 | |
| Magnesia, . . . . . . | 20.334 | 7.870 | 14.090 |
| Lime, . . . . . . . | 17.764 | 4.989 | |
| Protoxide of iron, . . . | 4.309 | 0.981 | |
| Protoxide of manganese, . . | 1.115 | 0.250 | |
| | 99.391 | | |

As the silicic acid contains almost exactly twice as much oxygen as the bases do, the following may be regarded as the formula:—

$$\left.\begin{array}{l}\dot{Mg}^3\\ \dot{Ca}^3\\ \dot{Fe}^3\\ \dot{Mn}^3\end{array}\right\}\dddot{Si}^2$$

This asbestus, therefore, has precisely the composition of pure augite, free from alumina; whereas that from the Tarentaise, analysed by Bonsdorf, has the composition of hornblende $\left(R.\dddot{Si} + \dot{R}^3\,\dddot{Si}^2\right)$, which, indeed, does not differ very much from the first. It appears, therefore, that the name asbestus does not belong to a determinate mineral, but to a condition into which several minerals can pass.—*Poggendorffs Annalen.*

7. *Geokronite.*—This new mineral species occurs in the mine of Sala in Sweden. Colour lead-grey, massive, but no cleavage; streak shining and metallic; opaque; hardness between calcspar and mica; specific gravity = 5.88. Analysis by Swanberg afforded the following ingredients, viz. lead, 66.452—copper, 4.544—iron, 0.417—zinc, 0.111—trace of silver and bismuth—antimony, 9.576—arsenic, 4.695—sulphur, 16.262 = 99.027. It thus appears to be a fifth combination of sulphuret of antimony and sulphuret of lead; the already described combinations being Jamesonite, Zinkenite, Plagionite, and Federerz. *Poggendorff's Annalen.*

8. *Meeting of the Geological Society of France at Aix.*—We have to announce that the extraordinary meeting of the Geological Society of France will this year be held at Aix (Bouches du Rhône), on the 4th of September, and that strangers, introduced by a member, may take part in the geological excursions and discussions.

9. *Sounding Sands.*—The late Sir Alexander Burnes, in his interesting work on Cabool, lately published, gives the following account of this curious phenomenon:—As we were now in the vicinity of Reg-Ruwan, or the moving sand, we made an excursion to it. It is a phenomenon similar to what is seen at Jubul-Nakoos, or the sounding mountain, near Too in the Red Sea. The Emperor Baber thus describes it:—"Between the plains there is a small hill, in which there is a line of sandy ground, reaching from the top to the bottom. They call it Khwaju Reg-Ruwan; they say that in the summer season the sound of drums and nugarets issues from the sand." The description of Baber, however marvellous it appears, is pretty accurate. Reg-Ruwan is situated about forty miles north of Cabool, towards Hindoo Koosh, and near the base of the mountains. Two ridges of hills, detached from the rest, run in and meet each other. At the point of junction, and where the slope of the hills is at an angle of about forty-five degrees, and the height nearly 400 feet, a sheet of sand, as pure as that on the sea-shore, is spread from the top to the bottom, to a breadth of about 100 yards. When this sand is set in motion by a body of people sliding down it, a sound is emitted. On the first trial we distinctly heard two loud

hollow sounds, such as would be produced by a large drum. On two subsequent trials we heard nothing, so that perhaps the sand requires to be settled and at rest for some space of time before the effect can be produced. The inhabitants have a belief that the sounds are only heard on Friday; nor then, unless by the special permission of the saint of Reg-Ruwan, who is interred close to the spot. The locality of the sand is remarkable, as there is no other in the neighbourhood. Reg-Ruwan faces the south, butthe wind of Purwan (bád i Purwan), which blows strongly from the north for the greater part of the year, probably deposits it by an eddy. Such is the violence of this wind, that all the trees in the neighbourhood bend to the south; and the fields, after a few years, require to be re-cleared of the pebbles and stones which the loss of soil lays bare. The mountains around are for the most part composed of granite, but at Reg-Ruwan we found sandstone, lime, slate, and quartz. Near the strip of sand there is a strong echo; and the same conformation of surface which occasions this is doubtless connected with the sound of the moving sand. In a late number of the Journal of the Asiatic Society of Calcutta, there is an extract of a letter from Lieutenant Wellsted, of the Indian navy, in which he describes the sounding mountain in the Red Sea, which has also been mentioned by Gray and Seetzen. There would appear, however, to be some variation in the kind of sound produced in the two places; but both are, I suppose, explained by the theory laid down by Mr James Prinsep regarding Jubl Nakoos, who says that the effect is there produced merely by "a reduplication of impulse, setting air in vibration in a focus of echo." At all events, we have at Reg-Rewan another example of the phenomĕnon to excite the curiosity of those interested in acoustics. Reg-Ruwan is seen from a great distance; and the situation of the sand is so peculiar, that it might almost be imagined the hill had been cut into two, and that it had gushed forth from the opening as from a sand-bag; the probability, however, is, that it has been brought together by the wind.

10. *Fossil Foraminifera in the Green Sand of New Jersey (America.)* —Prof. J. W. Bailey has brought to light the interesting fact, that a large portion of the calcareous rock defined by Prof. H. D. Rogers as the third formation of the upper secondary, is made up, at the localities where he examined it, of great quantities of microscopic shells belonging to the foraminifera of D'Orbigny, which order includes those multilocular shells which compose a large part of the calcareous sands, &c. of Grignon and other localities in the tertiary deposits of Europe. Since the minute multilocular shells above alluded to were discovered,

Dr Torrey and Prof. Bailey have together examined specimens of limestone from Claiborne, Alabama, and have found in them foraminifera of forms apparently identical with those occurring in New Jersey. None of this order, except the genus nummulite, have heretofore been noticed in our green-sand formation. In this connection we may also announce the interesting discovery recently made by Prof. Wm. B. Rogers, of a vast stratum of *fossil infusoria in the tertiary strata of Virginia.* It occurs about twenty feet in thickness beneath Richmond, and is found to be filled with new and highly interesting forms of *marine* siliceous infusoria. It would be interesting to have the specimens of the green-sand formation of the far west, collected by Mr Nicollet, examined, to see if infusoria or foraminifera may not be found in them.—*Silliman's American Journal of Science and Arts*, vol. xli. No. 1. p. 213.

### ZOOLOGY.

11. *Notice of a Memoir on the Organic Tissues in the bony structure of Corallidæ, lately read before the Royal Society.* By J. S. Bowerbank, Esq., F. G. S.—The author submitted small portions of nearly seventy species of bony corals to the action of diluted nitric acid, and thus obtained their animal tissue, freed from calcareous matter, and floating on the surface of the fluid in the form of a delicate flocculent mass. By the aid of the microscope, this mass was found to be pervaded by a complex reticulated vascular tissue, presenting numerous ramifications and anastomoses, with lateral branches terminating in closed extremities. There were also found, interspersed among these, another set of tubes, of larger diameter than the former, and provided, in may places, with valves; the branches from these larger vessels occasionally terminate in ovoid bodies, having the appearance of gemmules or incipient polypes. In other cases, masses of still larger size, of a more spherical shape, and of a brown colour, were observed attached to the membrane, and connected with each other by a beautiful net-work of moniliform fibres. Numerous siliceous spicula, pointed at both extremities and exceedingly minute, were discovered in the membranous structure of several corals; and also spicula of larger size terminated at one extremity in a point, and at the other in a spherical head; a form bearing a striking resemblance to that of a common brass pin. Besides these spicula, the author noticed in these membranous tissues a vast number of minute bodies, which he regards as identical with the nuclei of Mr Robert Brown, or the cytoblasts of Schleiden.

12. ***Silk Worms.***—The chief manufactures of Milan are silk and iron: of the former there are several establishments, and the culture of the mulberry is in consequence very extensive. Raumer quotes the following curious fact from Berger's book:—"24,000 eggs of the silk-worm weigh a quarter of an ounce; the worm lives from forty-five to fifty-three days; increases his weight in thirty days 9500 fold, and during the last twenty-eight days of his life eats nothing. For 739 lbs. of mulberry leaves, 70 lbs. of cocoons are obtained; 100 lbs. of cocoons give 8⅓ lbs. of spun silk; and one pound of cocoons will produce a single thread of 88,000 fathoms in length."—*Barrow's Tour in Lombardy*, p. 146.

13. *Snail Trade of Ulm.*—Ulm has not much traffic, the principal exports, as I understood, being snails, which are bred and fattened, and of which many millions are annually sent into Germany and other Catholic countries in Lent, where they are esteemed a great delicacy.—*Barrow's Tour in Lombardy, Tyrol, &c.*, p. 358.

### 14. *Notice of a Memoir, lately read before the Royal Society, on the ultimate Distribution of the Air-Passages, and of the Modes of Formation of the Air-Cells of the Lungs.* By William Addison, F. L. S., Surgeon, Great Malvern.

After reciting the various opinions which have prevailed among anatomists regarding the manner in which the bronchial tubes terminate, whether, as some suppose, having free communication with one another, or, as others maintain, by distinct and separate cells having no such intercommunication, the author states that, having been engaged in investigating, with the aid of the microscope, the seat and nature of pulmonary tubercles, he could never discover, in the course of his inquiry, any tubes ending in a *cul-de-sac;* but, on the contrary, always saw, in every section that he made, air-cells communicating with each other. He concludes from his experiments and observations, that the bronchial tubes, after dividing dichotomously into a multitude of minute branches, which pursue their course in the cellular interstices of the lobules, terminate, in their interior, in branched air-passages, and in air-cells which freely communicate with one another, and have a closed termination at the boundary of the lobule. The apertures by which these air-cells open into one another are termed by the author *lobular passages;* but he states that the air-cells have not an indiscriminate or general intercommunication throughout the interior of a lobule, and that no anastomoses occur between the interlobular ramifications of the bronchiæ themselves; each branch pursuing its own independent course to its termination in a closed extremity. Several drawings of the microscopical appearances of injected portions of the lungs accompany this paper.

15. *Loss and Recovery of Mr Swainson's Library.*—We have pleasure in recording the handsome and liberal conduct of Mr Maclear, the Astronomer Royal, and the Rev. Dr Adamson, the Presbyterian clergyman at the Cape of Good Hope, in recovering for Mr Swainson the zoologist his valuable library, which had been wrecked in the Prince Rupert at the entrance of Table Bay, when on its way to New Zealand. At the sale of the cargo at Cape Town, some cases of books injured by salt water were purchased by Mr Maclear, and, on examination, they were found to contain splendid works on natural history apparently the property of Mr Swainson. Subsequently, Mr Maclear and Dr Adamson succeeded, after considerable exertion, in obtaining from the auctioneers, &c. who had purchased them, the remainder of Mr Swainson's books, at a fraction of the value; and, instead of retaining their excellent bargain, these gentlemen have written to the original proprietor, informing him of the circumstance, and offering to forward the books to him.

## ARTS, &c.

16. *Speed of Travelling.*—The opening of the Strasburg and Basle Railway, which is about ninety miles in length, was celebrated recently by a great dinner at Mulhausen. An inscription on one of the walls of the room ran thus:—"In the year 1500, the journey from Mulhausen to Strasburg occupied eight days; in 1600, six days; in 1700, four days; in 1800, two days; in 1841, two hours." (The distance is about seventy English miles.)

17. GLASGOW.—*Improvement in Paving of Streets.*—We know of no improvements in the management of police matters equal to those which have been effected by the Commissioners of Police in the paving of the streets. It is only a few years since that department was transferred from the old statute-labour trust to their management, and at the time of transfer, the streets, with few exceptions, were nearly in a ruinous condition. The committee of statute-labour, and Mr Hume, their active superintendent, have, by their unwearied exertions, brought the system of the paving of the streets to the highest state of perfection. The results of their experience may afford useful information to public bodies or

others interested in the making or maintaining of streets. We will, therefore, here give a short account of them :—

*1st,* It is found that what is technically called ruble causewaying, although cheapest at first, is the most unprofitable description of material which can be laid where the thoroughfare is great.

*2d,* The common description of dressed causewaying is equally ineffective.

*3d,* Macadamised streets in towns, where the thoroughfare is great, are exceedingly unprofitable, and much more expensive to keep up than any other description of street.

*4th,* Whinstone (Trap) is a chalk compared to the granite (Porphyry) from Inverary Quarry. This was proved by the Trustees in Jamaica Street. Both descriptions of material were subjected for three years to an equal amount of tear and wear. On minute inspection, the whin was found worn down from 2½ to 3 inches, while the granite was so little defaced, that the marks of the tools used in preparing it were still visible.

*5th,* To make good paving, the material must consist of well-made square-dressed blocks, all of an equal depth, and must be free of holes and bumps on the tops and bottoms, and be carefully laid on a bed of sharp river sand, the stratum on which it is laid being previously prepared to the exact curvature required, or surface of the street in its finished state. The stones, being nearly smooth on the top, allow the carts to roll along, instead of bumping on the stones, which causes immediate destruction to the street.

*6th,* The Inverary granite is considered to be the most economical description of material which can be adopted, as, from its extraordinary durability, a street, when laid with it on the improved system, is calculated to last at least for fifty years.

The Commissioners of Police are about to have the High Street paved with this durable material. They and their superintendent of streets deserve the greatest credit for the great interest they bestow on the paving department.

18. *Eastern Method of Measuring Time.*—The people of the East measure time by the length of their shadow. Hence, if you ask a man what o'clock it is, he immediately goes into the sun, stands erect, then looking where his shadow terminates, he measures his length with his feet, and tells you nearly the time. Thus the workmen earnestly desire the shadow which indicates the time for leaving their work. A person wishing to leave his toil says, " How long my shadow is in

coming?" "Why did you not come sooner?" "Because I waited for my shadow." In the 7th chapter of Job, we find it written, "As a servant earnestly desireth his shadow."—*Roberts' Illustrations.*

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## NEW PUBLICATIONS.

We have received the following works, which we recommend particularly to the attention of our readers. It was our intention to have given an account of several of them, but we regret that for the present we are prevented doing so by want of space :—

*Explication de la Carte Géologique de la France. Par M. M. Dufrénoy et Élie de Beaumont, 4to. Darwin on Coral Reefs. Transactions of the Manchester Geological Society, Vol. I. Etudes Géologiques dans les Alpes. Par M. L. A. Necker, Tome* $1^{er}$. *Ueber den Jurakalk von Kurowitz in Mähren, von E. F. von Glocker, 4to. Recherches Physiques sur les Pierres d'Imatra, par G. F. Parrot, 4to. Oryctographie du Gouvernement de Moscow, par M. G. Fischer de Waldheim, folio. Monographie des Plantes Fossiles du Grès Bigarré de la chaine des Vosges, par M. W. P. Schimper, 4to. Parties* $1^{ere}$ *&* $2^{ème}$. *Nomenclator Zoologicus, auctore L. Agassiz. Fasciculus I., continens Mammalia, Echinodermata et Acalephas, 4to. De la succession et du Developpement des Etres Organisés, par M. L. Agassiz. Monographia Anoplurorum Britanniæ, with highly magnified figures of each species. By Henry Denny. A History of Infusoria, living and fossil, with plates. By Andrew Pritchard. A History of British Sponges and Lithophytes. By George Johnston, M.D., with plates. The Zoology of the Voyage of H.M.S. Beagle. Edited by Charles Darwin, Esq. No. 2, 3, and 4 of Part IV., containing Fish, by the Rev. L. Jenyns.* (The two following numbers will include the Reptiles and Amphibia; after the publication of which the present work will be completed.) *Illustrations of the Zoology of South Africa. By Andrew Smith, M.D., Nos. 12, 13, 14, and 15. Principles of General and Comparative Physiology. By William B. Carpenter, M.D., 2d Edition. On the Remote Cause of Epidemic Diseases. By John Parkin. Descriptive Catalogue of the Preparations in the Museum of the Royal College of Surgeons in Ireland. By John Houston, M.D., Vol. II. (Pathology). Anatomisch-Physiologische Untersuchungen ueber die Bothryocephalen. Von Professor D. F. Eschricht, 4to. Anatomische*

*Untersuchungen ueber die Clione Borealis, von Professor D. F. Eschricht,* 4to. *Elements of Electro-Metallurgy. By Alfred Smee. Parts* 1, 2, *and* 3. *Elements of Chemistry. By Robert Kane, M.D. Journey to Cabool. By the late Sir Alexander Burnes. A Series of Diagrams illustrative of the Principles of Mechanical and Natural Philosophy, and their practical application. Part I.* (Chapman and Hall). *Dunbar's Greek Lexicon (contains Appendix explanatory of scientific terms). Journal of the Asiatic Society of Bengal. Madras Journal of Literature and Science. The Quarterly Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland, No. 57—(contains Cunningham's Geognostical Account of Banffshire.)*

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## *List of Patents granted for Scotland from* 28*th December* 1841 *to* 16*th March* 1842.

1. To John Juckes of Lewisham, in the county of Kent, gentleman, "improvements in furnaces or fire-places."—28th December 1841.

2. To Montagu MacDonogh of Saint Albans Place, Middlesex, gentleman, being a communication from abroad, "improvements in spindles, flyers, and bobbins, for spinning, roving, twisting, and reeling all sorts of fibrous or textile substances, and in the application or adaptation of either or all of them to machinery for the same purposes."—4th January 1842.

3. To Thomas Joseph Ditchburn of Orchard House, Blackwall, in the county of Middlesex, shipbuilder, "certain improvements in shipbuilding, some, or all of which, are applicable to steam-boats and boats and vessels of all descriptions."—6th January 1842.

4. To Moses Poole of Lincoln's Inn, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in preparing matters to be consumed in obtaining light, and in the construction of burners for burning the same."—7th January 1842.

5. To William Petrie of Croydon, in the county of Surrey, gentleman, 'a mode of obtaining a moving power by means of voltaic electricity, applicable to engines, and other cases where a moving power is required."—7th January 1842.

6. To James Taylor junior, smith and engineer, Turner's Court, Glasgow, "a self-acting machine for driving piles and stakes, and for other such purposes, to be wrought by steam or other power."—10th January 1842.

7. To John George Bodmer of Manchester, in the county of Lancaster, engineer, "certain improvements in the construction of screwing stocks, taps, and dies, and certain other tools or apparatus or machinery for cutting and working in metals."—13th January 1842.

8. To **William Petrie** of Croydon, in the county of Surrey, gentleman, " improvements in obtaining mechanical power."—13th January 1842.

9. To **Alphonso Rene Le Mire De Normandy** of Red Cross Square, in the city of London, Doctor of Medicine, " certain improvements in the manufacture of soap."—13th January 1842.

10. To **Henry Hough Watson** of Bolton le Moors, in the county of Lancaster, consulting chemist, " certain improvements in dressing, stiffening, and finishing cotton, and other fibrous substances, and textile and other fabrics, part or parts of which improvements are applicable to the manufacture of paper, and also to some of the processes or operations connected with printing calicoes and other goods."—18th January 1842.

11. To **John Lee** of Newcastle-upon-Tyne, manufacturing chemist, " improvements in the manufacture of chlorine."—19th January 1842.

12. To **John Thomas Carr** of the town and county of Newcastle-upon-Tyne, being a communication from abroad, " improvements on steam-engines."—19th January 1842.

13. To **Robert Stirling Newall** of Gateshead, in the county of Durham, wire-rope-manufacturer, " improvements in the manufacture of flat bands, and in machinery for the manufacture of wire-ropes."—20th January 1842.

14. To **Christopher Nickels** of York Road, Lambeth, in the county of Surrey, gentleman, " improvements in the manufacture of napped fabrics." —27th January 1842.

15. To **John Jones** of the Smethwick Iron-Works, near Birmingham, in the county of Stafford, engineer, " certain improvements in steam-engines, and in the modes or methods of obtaining power from the use of steam, parts of which improvements are applicable to raising or forcing water, and for other purposes."—4th February 1842.

16. To **James Thorburn** of Manchester, in the county of Lancaster, machinist, " certain improvements in machinery for producing knitted fabrics."—8th February 1842.

17. To **Nathaniel Benjamin** of Camberwell, in the county of Surrey, gentleman, being a communication from abroad, " improvements in the manufacture of Type."—11th February 1842.

18. To **Louis Lachenal** of Tichfield Street, Soho, mechanic, and **Antoine Vieyres** of Pall Mall, watchmaker, both in the county of Middlesex, " improvements in machinery for cutting cork."—11th February 1842.

19. To **John George Bodmer** of Manchester, in the county of Lancaster, engineer, " certain improvements in machinery for propelling vessels on water, part of which improvements apply also to steam-engines to be employed on land."—14th February 1842.

20. To **George Mannering** of Dover, in the county of Kent, plumber, and **Henry Harrison** of Ashford, in the same county, plumber, " certain

improvements in the means of raising water and other fluids."—16th February 1842.

21. To William Baker of Grosvenor Street, Grosvenor Square, in the county of Middlesex, surgeon, "certain improvements in the manufacture of boots and shoes."—23d February 1842.

22. To George Haden of Trowbridge, in the county of Wilts, engineer, "certain improvements in apparatus for warming and ventilating buildings." —23d February 1842.

23. To Joseph Henry Tuck of the New North Road, Hoxton, in the county of Middlesex, engineer, "improvements in apparatus or machinery for making or manufacturing candles."—25th February 1842.

24. To Hugh Lee Pattinson of Bensham Grove, Gateshead, in the county of Durham, manufacturing chemist, "improvements in the manufacture of white lead, part of which improvements are applicable to the manufacture of magnesia and its salts."—25th February 1842.

25. To Matthew Allen of High Beech in the county of Essex, Doctor in Medicine, being a communication from abroad, "an improvement in producing uneven surfaces on wood."—2d March 1842.

26. To Stopford Thomas Jones of Stafford Place, Pimlico, in the county of Middlesex, gentleman, "certain improvements in machinery for propelling vessels by steam or other power."—2d March 1842.

27. To Joseph Drew the younger, of Saint Peter's Port in the island of Guernsey, confectioner, "an improved method of rolling and cutting lozenges, and also of cutting gun wads and other similar substances by means of a certain machine designed by him, and constructed of divers metals and wood."—7th March 1842.

28. To Joseph Garnett of Haslingden, in the county of Lancaster, dyer, and John Mason of Rochdale in the same county, machine-maker, "certain improvements in machinery or apparatus employed in the manufacture of yarns and cloth, and are also in possession of certain improvements applicable to the same, communicated from abroad."—8th March 1842.

29. To George Jarman of Leeds, in the county of York, flax and cotton spinner; Robert Cook of Hathersage, in the county of Derby, heckle and needle manufacturer; and Joshua Wordsworth of Leeds aforesaid, machine-maker, "certain improvements in machinery for spinning flax, hemp, and tow."—9th March 1842.

30. To James Ions of Newcastle-upon-Tyne, gentleman, "improvements in smelting copper ores."—10th March 1842.

31. To Julius Bordier of Austin Friars, in the city of London, merchant, being a communication from abroad, "certain improvements in preparing skins and hides, and converting them into leather."—11th March 1842.

32. To Richard Lawrence Sturtevant of No. 42. Church Street, Bethnal Green, in the county of Middlesex, soap manufacturer, "certain improvements in the manufacture of soap."—14th March 1842.

33. To William Hickling Burnett of Ravensbourne Wood Mills, Deptford Creek, in the county of Kent, gentleman, "improvements in machinery for cutting wood, and in apparatus connected therewith, part of which may be applied to other purposes."—14th March 1842.

34. To George Wildes of Coleman Street, in the city of London, merchant, being a communication from abroad, "improvements in the manufacture of white lead."—16th March 1842.

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## *List of Patents granted for Scotland from* 16*th March to* 23*d June* 1842.

1. To Moses Poole of Lincoln's Inn, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in the manufacture of plaited fabrics."—29th March 1842.

2. To James Hunt of Whitehall, in the county of Middlesex, gentleman, "improvements in the manufacture of bricks."—29th March 1842.

3. To James Hall of Glasgow, upholsterer, "improvements in beds, matrasses, and apparatus applicable to bedsteads, couches, and chairs."—30th March 1842.

4. To John Harwood of Great Portland Street, in the county of Middlesex, Esquire, "an improved means of giving expansion to the chest."—6th April 1842.

5. To James Andrew of Manchester, in the county of Lancaster, manufacturer, "certain improvements in the method or process of preparing or dressing yarns or warps for weaving."—6th April 1842.

6. To Edmund Morewood of Winchester Buildings, Great Winchester Street, in the city of London, Esquire, being a communication from abroad, "an improved mode of preserving iron and other metals from oxydation or rust."—7th April 1842.

7. To Henry Booth of Liverpool, Esquire, "improvements in the method of propelling vessels through water."—13th April 1842.

8. To William Brockedon of Queen Square, in the county of Middlesex, gentleman, "improvements in manufacturing fibrous materials for the cores of stoppers to be coated with India rubber, and used for stopping bottles and other vessels."—13th April 1842.

9. To Cristopher Nickels of the York Road, Lambeth, in the county of Surrey, gentleman, "improvements in the manufacture of plaited fabrics." —13th April 1842.

10. To James Smith of Deanston Works, in the parish of Kilmadock, and county of Perth, cotton-spinner, and James Buchanan of the city of Glasgow, merchant, "certain improvements applicable to the preparing and spinning of cotton, wool, flax, hemp, and other fibrous substances."—13th April 1842.

11. To Mathias Nicolas La Roche Barre of St Martin's Lane, in the county of Middlesex, manufacturer of cotton, "an improvement in the manufacture of a fabric applicable to sails and other purposes."—19th April 1842.

12. To Reuben Partridge of Cowper Street, Finsbury, in the county of Middlesex, engineer, "certain improvements in machinery or apparatus for splitting and shaping wood into splints for the manufacture of matches and other similar forms."—20th April 1842.

13. To Richard Dover Chatterton of Derby, in the county of Derby, gentleman, "certain improvements in propelling."—22d April 1842.

14. To Theophile Anton Wilhelm Count de Hompesch of Burich Castle, near Aix La Chapelle, in the kingdom of Prussia, "improvements in obtaining oils and other products from bituminous matters, and in purifying or rectifying oils obtained from such matters."—22d April 1842.

15. To John Venables of Burslem, in the county of Stafford, earthenware manufacturer, and John Tunnicliff of Burslem aforesaid, bricklayer, "a new and improved method of building and constructing ovens used by potters and china manufacturers in the firing of their wares."—25th April 1842.

16. To William Newton, of the office for patents, 66 Chancery Lane, in in the county of Middlesex, civil engineer, being a communication from abroad, "an improved machine or apparatus for weighing various kinds of articles or goods."—27th April 1842.

17. To Joseph Aitkinson of Braham Hall, near Harrowgate, in the county of York, farmer, "improvements in thrashing and winnowing machines."—4th May 1842.

18. To John Carr of North Shields, in the county of Northumberland, earthenware manufacturer, and Aaron Ryles of the same place, agent, "an improved mode of operating in certain processes for ornamenting glass."—6th May 1842.

19. To Henry Barron Rodway of Birmingham, in the county of Warwick, wine merchant, "improvements in the manufacture of horse shoes."—12th May 1842.

20. To Sir James Murray of Merrion Square, in the city of Dublin, Knight, and Doctor of Medicine, "an improved method of combining various materials in a manner not hitherto in use for the purpose of manure."—12th May 1842.

21. To John George Bodmer of Manchester, in the county of Lancaster, engineer, "certain improvements in machinery, or apparatus for clean-

ing, carding, roving, and spinning cotton, and other fibrous substances."—16th May 1842.

22. To **Peter Kagenbusch** of Wetter on Rhur, in Westphalia, in the kingdom of Prussia, dyer, now residing in the parish of Whitby, in the county of York, in England, "an improvement in the dyeing of wool, woollen cloths, cotton, silks, and other fabrics and materials."—17th May 1842.

23. To **Joseph Clisild Daniell** of Tiverton Mills, near Bath, "improvements in making and preparing food for cattle."—25th May 1842.

24. To **Robert Logan** of Blackheath, in the county of Kent, Esquire, "improvements in obtaining and preparing the fibres and other products of the cocoa-nut and its husk."—28th May 1842.

25. To **Thomas Henry Russell** of Wednesbury, in the county of Stafford, iron tube manufacturer, and **Cornelius Whitehouse** of the same place, "improvements in the manufacture of welded iron tubing."—28th May 1842.

26. To **Thomas Middleton** of Loman Street, in the borough of Southwark, and county of Surrey, engineer, being a communication from abroad, "an improved method of preparing vegetable gelatine, or size for paper, and also an improved mode of applying the same in the manufacture of paper."—6th June 1842.

27. To **John Railton** of Blackburn, in the county palatine of Lancaster, machine-maker, "certain improvements in machinery, or apparatus for weaving."—6th June 1842.

28. To **Thomas Hedley** of the town and borough of Newcastle-upon-Tyne, gentleman, and **Cuthbert Rodham** of Gateshead, in the county of Durham, millwright, "an improved apparatus for purifying the smoke gases and other noxious vapours arising from certain fires, stoves, and furnaces."—7th June 1842.

29. To **John Burnell** the younger, of High Street Whitechapel, in the county of Middlesex, manufacturer, "improvements in the manufacture of leaves or sheets of horn, commonly called lantern leaves, and in the construction of horn lanterns."—8th June 1842.

30. To **Otto Rotton** of Gracechurch Street, in the city of London' Doctor of Medicine, being a communication from abroad, "certain improvements in machinery, or apparatus for spinning cotton, wool, and other fibrous substances."—14th June 1842.

31. To **John Bould** of Ovenden, in the parish of Halifax, in the county of York, cotton-spinner, "an improvement or improvements in condensing steam-engines."—23d June 1842.

32. To **John Cox** of Gorgie Mills, Edinburgh, tanner and glue manufacturer, "certain improved processes of tanning."—23d June 1842.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

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## *The Glacial Theory and its recent Progress.* By LOUIS AGASSIZ, Doctor of Philosophy and Medicine, LL.D. of Edinburgh and Dublin, Knight of the Order of the Red Eagle of Prussia, Professor of Natural History in the Academy of Neuchâtel, &c. (Communicated by the Author.)*

THERE are few branches of natural science which have made such rapid progress as the subject of glaciers and the phenomena connected with them. When, about six years ago, I invoked, for the first time, the aid of a vast sheet of ice, covering the whole northern hemisphere, in order to explain, on the one hand, the transport of the erratic blocks distributed over the southern flank of the Jura, as well as the surface of the north of Germany, of England, of Sweden, and of Finland; and, on the other, the formation of rounded surfaces, which are often as smooth and as highly polished as the finest marble, and on which are traced fine parallel lines, resembling those produced by the *burin* of the engraver, I scarcely ventured to hope that any impartial minds would take the theory into consideration, so hostile was it to the ideas generally entertained up to that time. The researches on glaciers and their former extension, previously carried on by MM. Venetz and Charpentier, were scarcely known to naturalists; and those who were acquainted with them, considered the views promulgated as inadmissible. The glacial theory, *a fortiori*, was rejected as completely Utopian ; and, assuredly, there could have been no reason to complain of this, if it had been merely a speculation elaborated in the seclusion of a study, and apart from nature. In the midst of the varied productions which bear testimony to the activity of our epoch in all the domains of science, it is in some degree a matter of necessity for a scientific man to engage in such researches only as rest on a real foundation. Now, the glacial theory, however daring it might seem, was supported by numerous facts, which, though but little known, were not the less positive. There lay the germ of its future career ; and attention was speedily directed to these facts, as strange as they were of general occurrence. Observations were made, researches were carried on, the data of the system were verified, and discussions ensued on all sides. In the midst of this contest, the glacial theory has extended itself; and though it has not obtained the suffrages of all, and still involves many contradictions, yet the various facts belonging to it have become the subject of serious study, and, at the present day, there is no naturalist who does not recognise their importance. In this consists the advantage derived from it by science; an advantage of immense consequence, inasmuch as it promises to us prospectively the solution of one of the most beautiful and extensive problems in the history of the earth. It is no longer a mere theory, which is the subject of discussion ; the object of research is the connection of a whole series of phenomena, apparently very different, but whose relations are evident to all observers: these are the erratic blocks, the mounds of loose materials, the ancient moraines, the polished and striated surfaces, the *surfaces moutonnées,* the furrowings of rocks in a constant direction, and the whole group of remarkable facts which an illustrious geologist has designated by the characteristic name of the *erratic phenomenon.*

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* The publication of this valuable Memoir has been somewhat delayed in consequence of unforeseen circumstances.—EDIT.

Since the domain of observation has been fairly entered, the investigation has advanced with gigantic strides. The beauty of the subject, the vast field which it embraces, the exciting questions belonging to it, have awakened on all hands zeal, interest, curiosity, and ambition. There is now not an academy, not a scientific society, in which the erratic phenomenon has not been discussed and supported by new facts ; and such has been the activity displayed by the *savans* of every country, that the most succinct abstract of the works and memoirs on the subject which have appeared within the last two years, would greatly exceed the limits of an article like the present. M. de Charpentier, in his *Essai sur les Glaciers et le Terrain Erratique*, has described in detail the traces of ancient glaciers in the great valley of the Rhone and its lateral valleys, and also at a multitude of other points in Switzerland ; M. Studer has observed them on the southern side of the Alps ; and Mr Martins in the Grisons. The French geologists assembled at Grenoble in 1840 studied them in the Alps of Dauphiny, and made them the subject of their discussions at the meeting held at Lyons in 1841. The polished rocks, in particular, seem to be very distinct on Mount Cenis, where they have been detected by Mr Trevelyan and by Captain Le Blanc. MM. Renoir, Hogard, and Le Blanc have continued to observe the erratic phenomenon in the Vosges ; MM. Max Braun and Du Rocher have noticed it in the Pyrenees; and I myself have done so in the Black Forest. The Swiss and French Jura has in this respect been made the object of continued study by MM. Gressly, Guyot, and Desor, who have proved that the erratic blocks of the Alps extend far beyond the limits assigned them by MM. de Buch and Charpentier ; and, lastly, I have discovered erratic blocks, accompanied by polished and scratched surfaces, in a host of localities in the Alps, where they had not previously been known to exist.

The great phenomena of the north, although attributed to other causes, do not the less belong to the same subject ; and, since the investigations of MM. Alexander Brongniart and Sefström, they have been made the object of continued researches by MM. Böthlingk, Nordenskiöld, Eichwald, Durocher, Robert, Martins, Murchison, De Verneuil, and Kaiserling. Finally, the American geologists, also, have very recently noticed a vast net-work of polished rocks and erratic blocks in the United States.

But it is more particularly in Great Britain that the most unexpected discoveries have been made. Who could have supposed that in these islands, equally remote from the glaciers of the Alps and the ice of the north, traces of the action of ice should have been found! And, nevertheless, all the phenomena which indicate the former existence of glaciers are there just as evident, and just as well preserved, as in the neighbourhood of the glaciers of the present day. England likewise, —thanks to the activity and the zeal of her *savans*—already possesses quite a literature on the subject of glaciers; and it would be necessary for me to cite the names of most of the geologists of that country, were I to mention all the individuals there who have occupied themselves with this question.

The purely theoretical part of the erratic phenomenon has also attracted much attention; and the discussions to which it has given rise in many places, and particularly in the Geological Society of France, have contributed, on their part, to render the study still more interesting, by connecting it with the great problems of the cosmic system.

Passing from the geological domain, attention ought naturally to be directed to the glaciers of the present day, which are, in fact, the touchstone of the whole system; for, in order to be entitled to call in their intervention to explain the erratic phenomenon, it is necessary to know, even to the most minute details, their actual condition, and their action on neighbouring bodies. Now, although the researches of naturalists since the time of Scheuchzer and Saussure have made us acquainted with the general laws which preside over their formation and their metamorphoses, there is, nevertheless, a multitude of details regarding their internal structure and their modifications, as connected with the different seasons, the import of which is still unknown to us; and there is even a diversity of opinion among naturalists on some of the capital points. Since the publication of my *Etudes sur les Glaciers*, I have made many additional expeditions to the interior of glaciers with the view of answering certain objections made to my theory by natural philosophers. I have also resided a second time on the *Mer de Glace* of the Lauter-Aar and of the Finster-Aar, in order to study the structure of the ice at all heights and in all conditions of the atmosphere, and I hope to be able to continue these same researches still longer,—thanks to the munificence of his Majesty the King of Prussia, who has deigned to grant me his support in this undertaking.

A study of equally great importance, with reference to our theory, is that of the history of glaciers. The chronicles and the registers of the communes situated in the high valleys of the Alps contain scarcely any thing else but the recital of that long struggle between man and the rivers of ice; a history sad and melancholy when the enemy gains possession of the tract, but joyous and animated when he beats a retreat under the influence of a series of warm summers. The same influences and the same reactions must exist in the north of Europe, and the history of the formerly flourishing colonies of Greenland, when it becomes better known, will doubtless tell us of some of those extensive changes in the covering of ice at the North Pole.

In the present article I shall confine myself to some details regarding the observations I have made since the publication of my work; and, as these observations are of a very varied nature, some belonging more peculiarly to the domain of geology, and others to that of natural philosophy or of meteorology, I shall divide the subject into two sections, and intend, in the first, to treat of the erratic phenomenon properly so called, such as I have observed it in Britain. In the second, I shall communicate some of the experiments which I made on glaciers last summer, during my sojourn upon them, and in the course of my expeditions; and the whole will be terminated by a short narrative of our residence on the glacier.

## I. The Erratic Phenomenon.

I have already said that the most striking discoveries, in relation to the erratic phenomenon, had been made in Britain. The most important facts which I have observed may be arranged in three categories:—

1. The phenomena proper to the interior of valleys.
2. The dispersion of erratic blocks in plains, at great distances from their origin.
3. Parallel terraces.

1. *The phenomena peculiar to valleys* are almost identical at all places where they have been observed. When we study

the arrangement of erratic blocks of certain valleys in Scotland, we feel inclined to imagine ourselves in a valley of the Swiss Alps. I shall never forget the impression I experienced at the sight of the terraced mounds of blocks which occur at the mouth of the valley of Loch Treig, where it joins Glen Spean; it seemed to me as if I were looking at the numerous moraines of the neighbourhood of Tines, in the valley of Chamounix. These mounds or ramparts abut against the walls of the valleys, frequently forming at the mouths of the valleys a series of concentric belts, which occur precisely at those places where, supposing that the valley had at one period been occupied by a glacier, it ought to have terminated by the terminal moraines pushing against one another. Similar mounds are observed at the mouth of nearly all the valleys of mountainous countries. The most remarkable in the British islands are, in Scotland, those of the banks of Loch Awe and of Loch Etive, especially in the vicinity of Bunaw ferry; in England, those of the environs of Penrith and Kendal; and in Ireland, those which traverse the road that skirts the base of Cuilcagh to the west of Florence Court. The latter are more distinct than any that I have seen in the United Kingdom. The nature of the blocks composing these moraines, proves that they have not come from a great distance; but that they have been detached from the upper part of the valley, and transported by some cause to its extremity. It is among these blocks, sometimes of very considerable size, that we find the most angular. Now, if we consider the arrangement of the valleys, which proceed in all directions from the most elevated chains, and all of which present the phenomena of erratic blocks, and of more or less continuous moraines, we cannot for a moment doubt, that the cause of this transport has extended its effects by radiating from the interior of the elevated points of the district towards the plains. This is a fact of capital importance, for it proves that the phenomenon of transportation is to a certain extent a local phenomenon, inasmuch as it is connected with the neighbouring chains of mountains. Each great group of mountains in Britain has thus its system of erratic blocks limited to the extremities of its valleys. It is thus that Ben Lomond on the one hand, and Ben Nevis on the other, have their system of blocks independent of that of Ben Wyvis; Schihallien and the Grampians have equally theirs, as also the Pentland Hills, the Cheviots between Scotland and England, and the mountains of Cumberland and Westmoreland; lastly, the mountains which rise above Belfast, those of the county of Wicklow, and Cuilcagh, also seem to me to form so many separate groups, as regards the dispersion of their erratic blocks. But these relations of the blocks to the chains of mountains are only one of the peculiarities of their arrangement; it is indeed that very one which has been least insisted on, and with which the defenders of the theory of currents have the least occupied themselves; and yet they ought above everything to have endeavoured to explain it, because it includes facts the most contrary to their theory. How is it really possible to attribute to an eruption of the ocean, or to the effects of a continual *soulèvement*, the dispersion of different groups of erratic blocks arranged like a fan around each particular system of mountains? How, moreover, is it possible to conceive the existence of so many deep lakes, by whose beds, however, all these currents must nevertheless have passed, in order to perch the erratic blocks on the flanks of the mountains, rather than accumulate them in the bottom of the valleys?

A circumstance which further adds to the importance of these scattered blocks and continuous mounds, is, that the valleys in which they are met with have generally their walls more or less worn, rounded, smoothed, polished, and scratched. Now, this particular appearance is evidently to be attributed to the same cause which transported the blocks; for these two series of facts are everywhere intimately connected together.

It was in England and in Sweden that the first polished surfaces were observed, and these were everywhere attributed, until recently, to the action of great currents, without any regard being paid to the improbability of a current, or rather currents, spouting like springs from the top of all the valleys, and being sufficiently powerful to convey from their place of origin blocks sometimes of immense dimensions. It can easily be imagined, that, at a period when almost all geological phenomena were attributed to the action of water, no endeavour was made to search for another cause for the transport of erratic blocks. But if a comparison had been instituted between the polished surfaces and the effects produced by currents, very remarkable differences between them would have been discovered. As I have said elsewhere, rocks polished by glaciers of the present day present surfaces gently rounded, smooth, and continuous over large spaces, sometimes even perfectly flat, and passing uniformly over the most resisting portions of rocks as over the softest, without forming sinuosities or edges. They are, moreover, furrowed, in the direction of the movement of the glacier, by furrows more or less deep and rectilinear, and scratched by fine striæ, perfectly rectilinear, and evidently parallel to one another and to the furrows; and, when the latter offer deviations from the general direction of the valleys, it is in consequence of circumstances which it is easy to appreciate. Such are likewise the polished surfaces remarked at the bottom and on the flanks of the valleys which are encompassed by erratic blocks and moraines, even when they are no longer occupied by glaciers. But such are not the appearances exhibited by rocks worn by water; although smooth they are never polished, and their undulated and sinuous surfaces present hollows or irregular excavations wherever the nature of the rock favoured erosions; no portion of the surfaces worn by currents of water has exhibited to me those long rectilinear striæ so characteristic of the polishing of glaciers. These differences between the abrasion occasioned by glaciers and that caused by water, are very well explained by the difference presented by a current of water, which, while it bounds along, follows all the sinuosities of its bed, and a rigid mass of ice which advances slowly on account of its consistence. The conformity which I have already pointed out between the aspect of polished valleys whose flanks are charged with erratic blocks together with continuous mounds, and whose mouths are closed by concentric barriers of blocks, and the aspect of the valleys at present occupied by glaciers flanked by their lateral and terminal, ancient and recent moraines, and whose bottoms are polished, striated, and furrowed in the direction of the movement of the glacier; this conformity, I say, is the principal argument that has caused me to attribute to the existence of glaciers which no longer remain, the phenomena similar to those produced by the glaciers of the present day, and which we meet with in so many localities far distant from glaciers. The granitic and porphyritic rocks of many valleys in Scotland exhibit polishings equally brilliant with those at present observed on the slaty serpentines of the flanks of the glaciers of Monte Rosa. The most remarkable of these polishings that I have seen in Scotland are those of the banks of Loch Leven near Ballahulish, those of Glen Spean opposite Loch Treig, those of Bunaw Ferry, those of Schihallien, pointed out by Dr Buckland, and those of the neighbourhood of Edinburgh, where the late Sir James Hall was the first to observe them. In England, I have seen very fine instances between Shap and Kendal, and near Ambleside; and in Ireland, near Donaghadee and near Virginia. I shall not here enumerate the numerous localities where the polishing of the rock has disappeared entirely or in part, but where the form of the surface still attests its former existence, but will limit myself to the notice of a very curious fact of this kind which occurs on the left bank of Loch Treig. We have there a small hill of gneiss, of a rounded form, whose surface is no longer flat, but is traversed by veins of quartz, having perfectly smooth and striated surfaces. The contours of these veins are exactly conformable to those of the eminence; they are cut according to the same forms, but they are raised two or three inches above the surrounding rock. Precisely the same phenomenon is to be seen near the Hospice of the Grimsel, where veins of quartz, having a polished surface, traverse *roches moutonnées* of gneiss, whose surface has been rendered rough by the action of the atmosphere. The projection of two or three inches of the veins of quartz on the hill on the banks of Loch Treig, evidently indicates the amount by which the neighbouring surface has been lowered by the decomposition of the gneiss, since the time when the quartz was polished, and the whole eminence rounded.

Limestone rocks are equally polished in a multitude of localities; but the most remarkable instances are in Lancashire, and in the vicinity of Florence Court in Ireland. I have already said that the mounds of blocks, and the polished rocks, are every where found to be intimately connected together; but I do not mean to say that polished rocks are not met with where there are no mounds, and that mounds are not met with where there are no polished rocks; for it may happen that the rocks on which the ancient glaciers moved were, in certain localities, very little calculated to retain the polish, and that they may have lost their original lustre, while the moraines composed of rocks of the elevated portions of the valleys still exist on their flanks. It is thus that we see very beautiful examples of mounds and ancient moraines in Glenary, above Inverary Castle, although there are no polished rocks in the immediate vicinity; and it is thus that the magnificent moraines of Cuilcagh, near Florence Court, are not accompanied by polished rocks, in consequence of the facility with which the solid rocks of that locality are decomposed. On the other hand, we frequently see beautifully polished rocks without moraines; and this is especially the case with the very abrupt walls of narrow valleys, where the blocks have fallen down at the period of the retreat of the ice, and have reached the bottom of the valleys without resting on their flanks. In such instances, instead of presenting the form of moraines, the blocks are scattered irregularly over the surface. We have an example of this on the northern walls of Loch Etive.

Dr Buckland and Mr Lyell, receiving the interpretation that I have given of the facts quoted above, have also observed a multitude of localities which, joined to those that I have myself visited, form a very extensive net-work of traces of ancient glaciers, stretching over the largest portion of Scotland, a large part of Ireland, and the whole north of England.

II. *Dispersion of erratic blocks in plains.*—The phenomenon of erratic blocks and polished rocks is not limited to the chief groups of mountains, but is seen extending over the whole surface of the country where it presents itself; with this difference, that in the lower regions it assumes peculiar characters, different from those which I have described as belonging to mountains and their valleys.

Just as the erratic phenomenon is localized in the vicinity and in the interior of mountains, so does it exhibit uniform characters in the low country and in flat regions, covering vast tracts whose limits cannot with precision be referred to determinate centres. Blocks are seen extending from one mountain-chain to another, across considerable depressions of the surface; the accumulations of blocks transported from one place to another are no longer arranged in linear continuous series as in the valleys, where they form mounds or ramparts, which are moraines properly so called, but they are dispersed irregularly over the surface; the nature of the rocks mixed together in these accumulations no longer indicates an origin so limited as that of those moraines even which are at the mouths of the valleys. The dispersion of these blocks in different countries has not hitherto been described with sufficient care, and more particularly the erratic angular blocks with a rough surface have not been sufficiently distinguished from those that are rounded, polished, and scratched. There are, however, very important differences in this respect. In Switzerland, for example, we nowhere meet with *large* blocks, whether angular or round, whose surface is rubbed, polished, and scratched with rectilinear striæ, at great distances from their origin. Whatever may have been the cause of the transport of the erratic blocks of the Alps and the Jura, it always happens that the great mass of the large blocks have arrived there with rough surfaces and well marked angles, and that the pebbles of smaller dimensions alone are worn, rounded, polished, and scratched with rectilinear striæ. We may easily convince ourselves of this fact by walking along any part of the Jura chain. Another peculiarity worthy of attention is, that with us the large angular blocks generally repose on the more or less considerable masses of rounded and polished pebbles, and that these latter often pass into a fine sand or a clayey paste, which covers directly the polished surfaces of the solid rocks wherever the pluvial water, the melting snow, and the torrents resulting from it, have not caused them to disappear. This arrangement is very well seen in the environs of Neuchâtel.

The state of matters is by no means the same in Britain, and more particularly in Scotland. There the erratic blocks of all dimensions are, in certain circumstances, rounded, perfectly smooth and polished, and even scratched with rectilinear striæ, like the polished solid rocks—a feature only observed in the smaller pebbles in Switzerland. It is not to be understood that there are no large angular blocks in England and in Scotland; but there is this distinction to be made, that these blocks are generally not far distant from their natural position *in situ*, or that they are in small number compared with those which have evidently been acted on by a prolonged mechanical operation. But this is not all; far from being found lying at the surface of the ground, the large blocks are for the most part heaped up in a confused manner along with the smaller ones of all degrees of size, from the dimension of the smallest pebbles to the colossal volume of the largest erratic blocks, in a deposit of clay unequally distributed over all the low portions of the country. This deposit of clay, which is of very unequal thickness, and exhibits no trace of stratification, is what is termed *till* in Scotland. There is no locality in which I have been able to study the till more completely than at Glasgow, where the numerous works carried on in 1840 for the embellishment of the town had exposed it at many points; but everywhere it presents the same characters; the rounded, polished, and scratched blocks of very various dimensions, are every where indiscriminately mixed together in a marly or clayey paste. It is evident that it was with this mass, and in this mass, that the rounded and polished blocks have been transported during the whole journey which they have performed together, while the angular blocks have certainly not been rubbed in this manner. Mr T. Edington has, to the advantage of geologists, brought together, in his park at Glasgow, a magnificent collection of these polished and scratched blocks from the neighbourhood of the town.

Differences of this description in the facts observed at different localities, are an additional difficulty for all those who endeavour to explain them by means of currents. How, indeed, can it be now seriously pretended that a current can convey blocks in such a manner as to rub, round, and scratch one set of them, without their being heaped up according to their weight, and without their being covered by regular beds of finer materials, while the others remained angular, and retained their unequal and rough surfaces? These differences are very favourable to the glacier theory, which explains them in a manner that is quite natural.

Let us return to the glaciers of the present day, and we shall find in some of the phenomena presented by them the greatest analogy to the arrangement of erratic blocks, as I have just described it. When a glacier moves, it wears and rubs the bed on which it reposes; scratches the smoothed walls; triturates the detached masses which are interposed between the ice and the rock, and reduces them to sand or to an argillaceous paste; rounds the blocks, which are of an angular form, and which offer resistance to the pressure; and polishes completely those which have broad sides. At the surface of the glacier, matters proceed in quite a different manner. The fragments of rock which are detached from the neighbouring walls, and which fall there, rest upon the ice, and are at most thrown out to its edges. They thus advance with the glacier without being displaced, or at least without being rubbed against one another, excepting those which have become interposed between the rock and the ice, and they arrive at the extremity of the glacier with their angles entire, their edges sharp, and their surfaces irregular. Let us suppose, now, that, in consequence of certain circumstances, one of those immense glaciers charged with debris of rocks, such as the lower glacier of the Aar, or the glacier of Zermatt, should be melted, and it would result that all the angular blocks at the surface of the glacier would repose on the irregular mass of rounded debris which at present lies under the ice. Some of these blocks would likewise be carried to a great distance on rafts of ice, if the melting were sufficiently rapid to cause currents capable of floating large masses of ice charged with blocks. If we suppose, on the contrary, that a glacier or a large sheet of ice, like that which extends over the Col de St Théodule, were not commanded by numerous mountain peaks, then few or no angular blocks would fall on its surface, but the rounded blocks underneath would not the less be present. If we imagine that, in such a case, particular circumstances should also occur to cause the melting of the ice, there would then be found at the bottom an irregular deposit of rounded blocks, imbedded in the more comminuted materials, along with a few angular blocks above—in short, to the very letter, a sort of till. In this case, again, the melting of the ice would give rise to currents; and the more considerable these currents, the more they would contribute to operate farther on the materials already acted on by the glaciers, whether by conveying to a distance the lighter portions, and depositing them in regular stratification, or by penetrating them more or less, and giving them a false appearance of stratification. We actually observe something of a similar kind, on a small scale, in the oscillations that occur at the extremity of glaciers which sensibly advance and retreat; as, for example, under the extremity of the lower glacier of the Aar in the Grimselgrund; and, among the localities where glaciers no longer exist, I may cite the lower extremity of Loch Treig, and the neighbourhood of Muckairn, between Loch Awe and Loch Etive.

In order to explain the whole of the facts relative to the erratic phenomenon, in the limits within which they have hitherto been observed, it is sufficient to admit that the polar ice formerly extended as far at the North Pole as it now extends at the South. Thus, then, if the influence which has established the difference that exists at present between the extent of the ice at the two poles be a periodical influence, and if it describe one of those cycles of long revolution, which astronomers have been enabled to determine, we can not only conceive the possibility of a cold in our regions sufficiently intense to produce all the phenomena which I have described, but may even be able to determine its date and duration. I shall not reproduce here my general theory of the periodical refrigeration of our globe, for that would raise useless discussions in a field which the light of observation has not yet sufficiently illuminated; I shall only cite one fact, which tends to make us suppose that there really existed in the North a covering of ice, whose southern limits in Europe, at a certain epoch, reached about 50° N. Lat. I allude to that belt of blocks observed by Russian geologists (see the letter from M. de Meyendorf to M. Elie de Beaumont[*]), which extends across the centre of Russia, by N. Nowogorod towards Pinsk, as far as the confines of Silesia. It seems to me much more natural to regard this limit as an *isopagetic* line (*une ligne isopagétique*[†]), than as the southern limit of a current coming from the North, and charged with blocks; and this so much the more, because the phenomenon of the transport of the Scandinavian blocks extends not only into Russia and Germany, but reaches the eastern coasts of England. In attributing this effect to the action of a current, it would thus be also necessary to imagine a fan-shaped current; whereas a solid limit, during a certain time, of a covering of ice as extensive as that of the South Pole, obviates all the difficulties presented by such a phenomenon, such as the continuity and the regularity of the outlines, the uniform furrows of the polished surfaces of the North, the passage across the Baltic and the North Sea of the blocks which lie on the surface of Germany and of England, &c. In a second zone of blocks, more to the north than the first, and observed likewise in Russia, to the south of the White Sea, and of the lakes of Onega and Ladoga, we have a direct proof of the successive and slow retreat of this covering of ice, a second isopagetic line more remote than the first. If this covering of ice really existed, it must at last have retired beyond the northern limits of the British Islands, after having enveloped them partially or entirely; but so long as the northern ice had not retired to its present limit, the climate of Europe must have been colder than it now is, and, even when the primitive ice had abandoned the plains, groups of glaciers must have remained in all mountainous countries. Hence it appears natural, that during the retreat of this covering of ice, there must have been a period when the mountains of Scotland were the focus of numerous glaciers, which at first descended from their summits into the plains, but afterwards occupied only the interior valleys, before disappearing completely.

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[*] Archiv für Wissenschaftliche Kunde von Russland; von Erman. Berlin, 1841.

[†] Ισος παγετος, that is to say, of equal ice; in some sense the *isotherme* of the outline of the northern covering of ice; but as the limits of this ancient ice do not coincide with the isothermal lines, I have been obliged to propose a new name.

There would thus be two very distinct periods to be particularized in the epoch of the existence of ice in the north of Europe,—that during which the general covering enveloped the region, and that when glaciers existed only in the high valleys. The dispersion of erratic blocks over great spaces, across considerable depressions of surface, the formation of the till, the furrowing and uniform striation of the polished rocks of Sweden and of Finland, seem to me the chief phenomena which have been produced by the northern covering of the epoch of ice. The differences which exist as to the erratic phenomenon between the north and the centre of Europe, appear to me to be susceptible of easy explanation by the differences of latitude and of the configuration of the surface. In Britain, the ice, at the time of its greatest extension, seems to have covered completely great tracts of country, and consequently rendered the fall of blocks on its surface, if not impossible, at least extremely rare; so that the great mass of the blocks was necessarily buried under the ice, and was therefore subjected to all the effects of a gradual and long-continued trituration, just as is observed beneath the glaciers of the present day. Mountains of considerable elevation in Scotland—Schihallien, for example—have their summits as polished as their flanks; whereas in Switzerland there exists a limit, at about 9000 feet,* in the centre of the Alps, above which the summits are no longer polished, but where the rugged peaks present a very striking contrast to the lower surfaces, which are polished, or, at least, *moutonnés.*†

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* All the measurements given in this paper are in *pieds de Roi,* or French feet; and the temperatures are all indicated in centigrade degrees, unless where other measurements or other degrees are specially mentioned.

† *Vide* the Comptes Rendus de l'Academie des Sciences, 1842; tome 14, p. 412.

In the exterior chains of the Alps, the polishing does not reach to a greater height than 6000 or 7000 feet. It cannot be doubted, that this limit, which is so well marked, indicates the level of the bed of ice at the epoch of its greatest thickness. The rugged peaks, which exceed that height, thus rose like islets in the midst of this sea of ice, and the blocks which were detached from them fell on the surface. Not being confined in narrow valleys, but the whole vast sea of ice being open to them, these blocks were not liable to be knocked against one another in their progress towards the lower districts, and it is thus that they could be transported as far as the Jura, with their surfaces rough and their angles prominent; whereas, the matters which were beneath the ice, were triturated, polished, rounded, and scratched. Now, if in Switzerland, the limit of the great mass of ice extended as high as 9000 feet in the Alps, and if it oscillated between 4000 and 5000 feet in the Jura which no longer presents glaciers, what is more natural than to admit, taking into account the geographical portion of the localities, that, in Scotland, the great proportion, if not the whole, of the surface, was entirely under ice during the whole duration of the glacial epoch. Hence the majority of the detached blocks of the Scotch mountains must have been transported under the ice, and consequently rubbed, rounded, polished, and scratched. I say the majority, for it is probable that some were detached when the ridges were free from ice, and when the valleys alone were occupied by glaciers; and these latter have necessarily remained more or less angular, and have retained their rough surfaces, just like the blocks of the moraines of the glaciers of the present day. Foreign blocks, whose origin is not British, and which were doubtless transported on the surface of the great sheet of ice, or on rafts of ice at the period of its dissolution, ought to be angular, and, for the most part, are so in reality. In this way, the form of erratic blocks implies, in some degree, at first sight, their mode of transport. I am able to add, as a confirmation of what I have said as to the form of the erratic blocks of Scotland, that the blocks of the Jurassic rocks, which we meet with in the diluvium of the interior valleys of the Jura, are all rounded; a proof that they have been transported under ice; and in fact this ought to be the case, because the polished rocks furnish us with the proof that the sheet of ice covered nearly all the summits of the Jura.

The melting and the retreat of the ice seem to me to have caused, at different times, according to the climatological circumstances, all those deluges, more or less extensive, of which records have been sent down by tradition and history. It is doubtless to these inundations that we must also attribute the dislocation of a large portion of the moraines, especially of those that, by their position, were not beyond the reach of the currents, which, by acting on the detritus at the bottom of the sheets of ice and of the glaciers, have given it, in many localities, a stratified appearance; so much so indeed, that we might be deceived as to the origin of these detrital matters, and attribute their rounded form to the effects of great currents, as has often been erroneously done. I do not believe that I deceive myself when I affirm, that whenever rounded blocks, lying in accumulations of gravel, stratified or unstratified, are scratched by long rectilinear striæ, their aspect is due to the action of the rubbing of glaciers against their beds; and that currents, in acting subsequently on these same matters and rolling them, could not but cause these characteristic marks to disappear by the friction. I therefore regard the rarity of scratched pebbles and blocks, in a deposit of stratified gravel, as a proof of a longer transport by water, and their total absence as a proof of an action due exclusively to currents; whereas, the complete absence of stratification in the accumulations of gravel and blocks uniformly rounded and scratched, seems to me to be the exclusive effect of glaciers. Lastly, these characters may be combined when such accumulations are the combined effect of the two causes, as may have been the case on maritime shores, where the glaciers of neighbouring mountains terminated at the coast. It must likewise not be forgotten, that sometimes small lakes are formed on the flanks of glaciers, in which the matters triturated by the glacier are deposited in regular beds, without being carried very far. It is of consequence to keep all these facts in view, when we study the formations which geologists have termed *diluvium*, and whose various phenomena have hitherto been erroneously attributed to one single cause,—currents.

It appears to me probable, according to the facts which I have been able to combine in considering this question, that the organized beings of our epoch were created successively, after the commencement of the retreat of the ice. Wherever the surface of the ground made its appearance between the glaciers, under the influence of a milder climate,—wherever, yielding to the temperature, the ice produced pools of water,—the development of organized beings might take place; and direct observation has already confirmed what the theory required. Mr Smith of Jordanhill was the first to point out in the post-tertiary clays, which are superior to the till (that is to say, which have been deposited posteriorly to the accumulation of those masses of gravel and rolled blocks in the mud under the ancient glaciers), numerous fossils of species that no longer exist similarly associated on the neighbouring coasts; he has even ascertained the identity of some of those shells with species which have hitherto been observed only in the Arctic seas. A fact so unexpected did not fail to excite my curiosity in a high degree, and I have ever since been unremitting in my endeavours to compare these fossils with living species. Assisted by a collection of living species from Greenland, which I owe to my friend Professor Eschricht of Copenhagen, I have not only confirmed the first impressions of Mr Smith, but have further found among the fossils of these clays a much larger proportion of Arctic species than could have been expected. Extending this species of research to the most recent fossiliferous deposits of other parts of Europe, I have every where met with a certain proportion of species whose types no longer exist in a living state in the neighbouring seas, but at 12° or 15° of latitude more to the north. Thus, while the shells, which are now found in lat. 65° to 70° on the coasts of Iceland and Greenland, where the mean temperature is several degrees below zero (32° F.), lived in lat. 55° to 60° on the coasts of Scotland and of England, where the mean temperature at present is + 8° (46°. 4 F.); the species of the coasts of England and of the British Channel which now live in lat. 50°

to 55°, lived in Sicily in lat. 35° to 40°; or, in other words, when the climate of Greenland extended its frosts beyond Scotland, when the mean temperature of the British Islands, in place of being above + 8° (46° .4 F.) cent., scarcely reached zero, the present climate of England, and of the north of Germany, prevailed in those parts of Europe which are now the warmest, and where the mean temperature exceeds + 16° (60° .8 F).

I shall afterwards publish the details of these observations, when they embrace a basis sufficiently complete to form an intimately connected whole; it is sufficient, for my purpose at present, to have indicated the principal results of these researches, which confirm the opinion of the former existence of a climate much more rigorous than that which now exists in Europe, by proofs independent of those derived from the traces of ancient glaciers. Now, a climate so different, could not have existed without exercising a marked influence on organic life; and it is thus that the Arctic faunas, in our temperate regions, confirm as fully the existence of ancient glaciers, as the presence of these same glaciers explains the existence of northern animals; and, nevertheless, the facts which establish the presence of the one, have nothing in common with the facts which prove the presence of the other.

III. *Parallel Terraces.*—The third order of facts is that of parallel terraces. This phenomenon is too well known to require description. Justly valued memoirs have delineated these terraces in full detail; and the publications of Macculloch, of Sir T. Dick Lauder, and of Mr Darwin, are so well known, that I shall confine myself to the task of establishing the connection which I conceive there is between these facts and the existence of glaciers in Scotland, without discussing the various theories that have been proposed for their explanation. When I visited the parallel roads of Glen Roy with Dr Buckland, we were convinced that the glacial theory alone satisfies all the exigencies of the phenomenon; and, as this locality is the best known of all those where parallel terraces have been observed, I may limit myself to this example for the explanation of all the others. The flanks of Glen Roy and Glen Spean exhibit horizontal and continuous terraces one above the other, which preserve npletely their parallelism throughout their whole extent, notwithstanding the numerous sinuosities of the valleys. The upper part of Glen Spean, as far as the part opposite Loch Treig, presents only one of these terraces, which surrounds Loch Laggan, and is prolonged round Loch Treig. This same terrace extends along the left side of the valley nearly down to the Bridge of Roy; on the right side it penetrates into Glen Roy, of which it makes the round, following all its sinuosities, and, continuing always at the same level along the lower part of Glen Spean, it terminates, on this side also, opposite the Bridge of Roy. It is worthy of remark, that the level of the *col*, separating the valley of the Spean, which runs to the west, from that of the Spey which runs to the east, is only a few feet above that of this first terrace. As far as Loch Treig, the valley of the Spean is surrounded by one terrace only; but lower down there are two others at different levels; which follow, in the same manner, all the sinuosities of the lower part of Glen Spean and of the whole of Glen Roy; which, taken at equal elevations, correspond perfectly on both sides of the valley; and both of which likewise terminate at the same point as the first, but at different levels, viz. near the Bridge of Roy. The first, or the least elevated of these three terraces, is 972 English feet above the level of the sea; and, as it is horizontal, its height above the bottom of the valley depends on the point of observation. The second is 212 feet above the first, and the third 82 feet above the second. It is to be remarked, that the two upper terraces make the round of Glen Roy, whereas, in Glen Spean they do not extend higher than the opening of the valley of Loch Treig. I noticed them on the left side of Glen Spean between Loch Treig and the Bridge of Roy, as well as on the flanks of Glen Roy; and I mention this particularly, because they are not indicated at that point in the maps which represent their position (Plate IV. Fig. 1). It is evident that these terraces indicate levels of water. The next enquiry is, if the barriers which restrained these lakes have disappeared, or if the valley has been elevated at different times above the level of the water? The perfect horizontality of these terraces, at three different levels, appears to me irreconcilable with the idea of a repeated soulèvement of the surface. The ablation of a rocky barrier seems impossible, without the influence of a cause which would, at the same time, have occasioned the disappearance of terraces having so little consistence; whereas, in a country which presents so many traces of ancient glaciers, the supposition of a great glacier, descending from Ben Nevis, and shutting up the valley of the Spean, by resting on Moeldhu, which is opposite, combined with the influence of a glacier issuing from Loch Treig, and which would bar the valley a second time at that height, would explain all the facts. The glacier of Loch Treig, of inferior size to that of Ben Nevis, would, first of all, be lowered at two different times, after having for a certain period maintained the water contained between the two glaciers at the level of the two upper terraces. During these two lowerings, the waters would run to the east, proceeding by the valley of the Spey, owing to the inconsiderable height of the *col* which separates that valley from Glen Spean. Whenever the glacier of Loch Treig disappeared completely, the water would be able to extend to the end of Glen Spean, and likewise invade Loch Treig; which explains the continuity of the lower terrace, while the two upper ones terminate abruptly opposite Loch Treig. Afterwards, when the great glacier of Ben Nevis no longer reached Moeldhu, the waters would run to the west, and water would remain only in the hollows which are now occupied by Loch Treig and Loch Laggan. The sudden termination of the three terraces, on the two sides of Glen Spean near the Bridge of Roy, will likewise be understood from this explanation. The supposition now made is confirmed by a fact which there is no other mode of accounting for: viz. that the bottom of Glen Spean in front of Loch Treig is not only polished with that polish characteristic of glaciers, but is moreover scratched transversely, that is to say, at right angles to the direction of the valley, by a cause which evidently proceeded from Loch Treig. I do not believe that a locality exists, where the facts indicate, in a more special manner, the cause which has produced them. The horizontal terrace of Glen Gloy is susceptible of a very natural explanation by a glacier issuing from the valley of Loch Arkeig, crossing Loch Lochy, and damming up Glen Gloy above Low Bridge. This supposition would also clear up the difference of level between the terrace of Glen Gloy and those of Glen Roy, and would obviate the necessity of imagining *soulèvemens* of the neighbouring valleys, which communicate in the same manner with the ocean, and do not nevertheless exhibit any trace of terraces.

In following up these facts in all their variety, we are easily enabled to explain the numerous terraces which we meet with in Scotland, by supposing barriers of ice at the mouths of the valleys; whether it was that the lateral valleys closed them by their glaciers, as at the Bridge of Roy, or that the waters of the sea, by heaping up ice on the coasts, offered a temporary obstacle to the running off of the waters of the land, or intercepted large sheets of salt water. The presence of an Arctic fauna, in the deposits superior to the till, which might be formed in these creeks of the sea, would thus present nothing but what is quite natural.

In order the better to understand the explanation I have given of the phenomena of Glen Roy and of Glen Spean, I shall now endeavour to compare them with what would take place in the valley of Chamounix, if the *Glacier des Bosons*, or that of Taconay, by extending a little farther than it now does, had barred the valley in such a manner as to interrupt the passage of the waters of Arve, and force them to flow off by the valley of Trient (Plate IV. Fig 2). A lake would thus be formed above Chamounix, of which the level would leave some traces on the flanks of the valley; but, when the glacier was no longer so elevated as the height of the *Col des Montets*, the water would flow to the west, as at present, forming terraces as often as it was maintained for a certain time at a fixed height. To render this comparison still more striking, let us suppose the case where a second glacier, as, for example, the *glacier des Bois*, also shut up the valley; then, as in the instance of Glen Roy, there would be between the two glaciers a lake, whose waters would first of all descend to the east over the least elevated glacier, and would not acquire their natural direction to the west until the two glaciers disappeared, and the bottom of the valley was free. Ice, cast on the coasts of Holland at the present day, and interrupting the dispersion of the sand transported by the Rhine and by the Scheldt, would every where reproduce the phenomenon of stratified fossiliferous deposits and of parallel terraces, such as we find so frequently in Scotland.

It was in Scotland that I acquired precision in my ideas regarding ancient glaciers. The existence in that country of so considerable a network of these traces, enabled me to appreciate better the geological mechanism of glaciers and the importance of many facts of detail observed in the neighbourhood of those which now exist. There, also, I was able to appreciate the influence exercised on these phenomena by the vicinity of the sea, and to distinguish the effects due solely to the waters of the sea from those where the ocean and glaciers were in contact, and from those produced where the glaciers never reached the sea. Who would now deny that glaciers formerly possessed in many localities an infinitely greater extension than at present? Who would reject the idea that the cause of this cold has been general, and attribute to local causes effects so diffused over the surface of the globe? And if these conclusions must be admitted, who does not perceive that physical theories must undergo some modifications before they can be made to embrace the whole phenomenon which I have described? It is thus that the study of facts in detail always reacts on general ideas; while, in their turn, theories, in order to be supported, force their partisans to the investigation of new facts. The activity of mind that is engendered by the contests to which researches of this kind always give rise, is probably the greatest enjoyment afforded us in this world.

## II. RESEARCHES ON EXISTING GLACIERS.

The chief question in the glacial theory is undoubtedly that of the motion of glaciers; it is the keystone of the arch of the edifice, for it includes at the same time the solution of the phenomena of the present time, and of those which occurred before our epoch. It is well known that opinions have hitherto been divided as to the progression of glaciers, and that many geologists, refusing to admit the theory of dilatation, still defend the opinion of Grüner and of Saussure, which maintains that a glacier slides along its bed.

In my *Etudes sur les Glaciers,* I have entered fully into the reasons which caused me to reject the theory of Saussure, and to deny the melting action of terrestrial heat on the bottom of a glacier. This negation leads to a consequence of great moment for the theory of the motion of glaciers, the immobility of glaciers in winter; and it does so in the following manner:—If, as I suppose, the motion of glaciers be really produced by infiltration and the daily congelation of the rain-water or of the water resulting from the melting of the superficial ice, glaciers ought to be stationary so long as there is no water at their surface, consequently during the whole winter, because they are destitute of water at that season. If, on the contrary, it be the terrestrial heat, which, by melting glaciers at their base, produces partially or entirely their sliding movement, this action ought to be perceptible at all periods of the year, independently of the seasons and of the oscillations of the temperature of the atmosphere; and if this last be the case, glaciers ought to furnish water during the whole year, in winter as in summer. Saussure himself followed this train of reasoning; and in order to arrive at a solution of the question, he went during winter to the valley of Chamounix, and witnessed the escape of pretty considerable streams from the arched terminations of glaciers, although they were less abundant than in summer. Thenceforward the point appeared to him to be settled. It was too cold for the sun to have the power of melting the ice; elsewhere, the whole country was buried under a thick crust of snow. It was, therefore, there could no longer be any doubt, the terrestrial heat which melted the glaciers beneath. Nevertheless, one reflection could not but present itself: if we consider that glaciers generally occupy the bottom of deep valleys, we must admit that it is there where the springs which circulate in the interior of the mountains ought to have their exit. The presence of water does not then prove absolutely that it results from the melting of the glacier. On the other hand, if the action of terrestrial heat exercise its influence in winter as in summer, it ought to do so on all glaciers, and not one of them should be dry. Further, the water of glaciers has a peculiar character, which distinguishes it from spring-water; it is always mixed with earthy matter, which it carries off from the bed of mud or of gravel that is interposed between the glacier and the rock. If it be really glacier-water which escapes from glaciers in winter, that water ought to possess these qualities; whereas, if it be spring-water, it ought to be clear and limpid, from where it begins to run.

It was of importance that I should myself examine on the spot the value of his reasoning; and it was with this view that I visited, in company with M. Desor, the glaciers of the Bernese Oberland, at the beginning of the month of March last year (1841), a period when winter reigns supreme in the High Alps. We selected the glaciers of the Aar and of Rosenlaui as the field of our observations, and proceeded towards the Hospice of the Grimsel, by ascending the fine valley of Hassli, which was still entirely covered with snow. The Aar, above Meyringen, was reduced to a small stream, the water of which was much clearer than in summer,—a circumstance which led us to suppose that it was chiefly, if not entirely, composed of spring-water. Bridges of ice traversed the bed of the river at a multitude of places, and as they were very thick, we were able to cross them in perfect safety. Above the height of 4000 feet, there only remained but a very small thread of water. The snow had frequently a thickness of 10 feet; the great and beautiful cascade of the Handeck had disappeared; and, on examining the beds of gneiss forming the precipice over which the water is precipitated in summer, we saw with astonishment, that they had partially preserved their projecting angles, and were not at all worn, as we should have supposed they must have been, especially when we remember that the Aar transports an enormous quantity of gravel. I insist on this fact, because it proves that if the water which has struck with violence against these rocks for a long series of ages, has not succeeded in rounding them, it is impossible to understand how we can attribute to the action of water the polish and the rounded form of the flanks of this same valley, at places where the inclination is much less considerable, and the valley much less contracted, and where the current must have acted with a proportionably much smaller degree of violence. It may perhaps be objected that these projecting angles are the result of a recent fall of rock, as in the case of the Falls of Niagara, and that the water has not yet had time to round them; but we must attend well to the fact, that we have not here to do with stratified rocks, like those of Niagara, but with a very compact gneiss, which is very obscurely stratified, and which, consequently, is not at all liable to breaking down.

At the Grimsel the snow was still thicker than at the Handeck; the bed of the river was entirely filled, and the glacier, as well as its moraines, were concealed under the snow. We were not deterred, however, from visiting the *Hôtel des Neuchâtelois*, which is situated at a distance of more than two leagues above the extremity of the glacier, and at a height of 7500 feet. I borrow from the yet unpublished narrative of our expedition by M. Desor, some passages which will give an idea of the aspect of those regions in winter, and of the obstacles against which we had to struggle, and will, at the same time, report the observations we were enabled to make.*

"The distance from the Handeck to the Grimsel is only two leagues, but as the snow became always more abundant as we advanced, we could not hope for an easy road. The most difficult places were the woods of young pine trees. The bed of snow which covered them was unequally distributed, and when we accidently stepped near one of the trees, we were immersed up to the waist; an occurrence which, on each occasion, caused very fatiguing shocks. At Raetherischboden, the last enlargement of the valley, a small thread of water occupied the bed of the Aar; but the water was so pure that, from the first, we supposed that it must come from some spring, and not from the glacier. It did not carry along with it any of those plates of mica whose presence gives to the water of glaciers that sparkling appearance and that milky tint which characterize it.

"The last league seemed to us the longest. The heat and the difficulties of the road had so exhausted us, that we were obliged to repose several times in order to recover our breath. At length we heard the barking of the dogs of the Hospice. It was, as it were, a friendly voice calling us to be of good cheer. We felt our vigour reanimated by this encouraging sound, and in a few moments afterwards, we saw making their appearance, on the mountain which rises above the Grimsel, the keeper of the Hospice, accompanied by his fine Newfoundland dog, Barry.

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* The narrative from which M. Agassiz extracts this passage has been published, since the present memoir was written, in the *Bibliothèque Universelle de Genève*, No. 76. We had intended to transfer it to our pages, in whole or in part; but this is now rendered unnecessary.—Edit.

"A small traffic of exchange is carried on between the Valais and the Hassli, which is not entirely discontinued during the winter, and of which the Grimsel is the warehouse at that season. The Hasslians bring their cheese, the Valaisans their wine, their brandy, and various kinds of provisions, among others, rice, which comes from Italy by the Simplon or the Gries. The two parties stop at the Hospice, sleep there, and are at home next day, carrying with them cheese, if they descend to the Valais, and wine and brandy, if they return to the valley of Hassli. It is for the purpose of facilitating this communication that the keeper of the Hospice is bound to have a man and two dogs at the Grimsel during the whole winter, and likewise to place picquets on the whole mountain of the Grimsel between the Hospice and the Valais, to point out the path to travellers.

"It is unnecessary to remark, that, to allow of this commerce being carried on in winter, the weather must not be too severe, for it would be madness to attempt such a journey in the midst of snow or wind. Thus the Grimsel, at such a time, is altogether solitary, and the keeper told us, that during the winter of 1839–40 he had passed thirty-five days without seeing a human figure. 'This long isolation,' he added, 'seemed to me so painful, that, on perceiving the first traveller who passed the Grimsel, I threw myself on his neck, embraced him, and offered him a bottle of wine.' The dogs here are at least as important as the men for watching, on account of the extreme delicacy of their senses, and especially of that of smell. All the guides assert that in serene weather, and especially in winter, they detect the presence of a man at the distance of a league, and Jaun assured us, that an hour before our arrival, he had already remarked, from the inquietude of Barry, that some one was approaching the Hospice.

"Those who have visited the Grimsel in summer will doubtless remember, that, in order to enter the vestibule, it is necessary to ascend a stair about seven feet high. Now, to give an idea of the quantity of snow which was accumulated round the house, it is sufficient for me to state, that, in place of *ascending* to the vestibule, we *descended* there by a stair which Jaun had cut in the snow. The lake of the Grimsel was completely invisible, for an uniform bed of snow extended over it, and did not even allow its limits to be recognised. But the stream which issues from it, although more abundant than we should have expected, was only visible in the interior of the building, whose floor it traverses. It is well known that the spring which gives rise to this lake, like many other springs in the Alps, is a thermal one. We were not, therefore, astonished to find the water at some degrees above zero (32° F.) We were subsequently assured by M. Zybach that the lake never freezes, whatever the degree of cold may be, and that even the enormous bed of snow does not rest on a crust of ice, but extends over the surface of the water; and he added, that, on sinking a rod through the snow, the water spouted to the surface. We were entirely ignorant of this peculiarity, or we should not have failed to investigate it. Perhaps other naturalists may find an opportunity of doing so.

"The first thing we did on our arrival was to place our thermometers. M. Agassiz sank a minimum thermometer in a hole pierced in the snow to a depth of five feet, while another thermometer was fixed at the surface. We were astonished to find that at seven o'clock in the evening the temperature of the air was not lower than $-4°$ C. ($+$ 24° 8 F.), although the sky was perfectly serene. We were very desirous to ascertain the humidity of the atmosphere, but, to our great vexation, our hygrometer had been broken during our journey.

"We went to bed at an early hour, immediately after supper; having decided to start for the *Abschwung* the following morning at four o'clock. Our guides made us still hope that perhaps the cold of the night might be sufficiently great to cause the snow to bear us, which would considerably facilitate our walk. At three o'clock we were up, and while Jacob prepared the coffee, we examined our thermometers, and saw, to our great disappointment, that the cold was far from being so intense as we imagined; for, as during the previous summer, we had on serene nights, seen the thermometer descend to −5° and 6° (+23° and 21°.2 F.), we expected to find it now at −12° or 15° (+10°.4 or 5° F.), or even lower. In place of that, the temperature of the air was −2° (+28.4 F.), and the thermometrograph which was sunk in the snow indicated −3° (+26°.6 F.). To what cause are we to attribute this singular state of the temperature? We asked Jaun if it happened often that the nights were so mild, and he replied, that, for a long time past, it had not been so cold as formerly. Notwithstanding this temperature, the snow bore us while we descended the declivity which leads from the Hospice to the bed of the Aar. We now looked forward to the prospect of proceeding with light steps over the hardened crust, and of scaling the edge of the glacier with equal facility; but we had scarcely advanced a few paces in the valley, when we arrived at a place where the crust gave way under our feet. Under this superficial crust the snow was very fine, dry, and powdery, and we sank sometimes with the one foot, sometimes with the other, generally up to the knee. We attempted to regain the flank of the valley, but this was still worse. It was necessary for us, nolens volens, to be satisfied with advancing very slowly. We had to endure a double punishment; in the first place, on account of our impatience, which could not be reconciled to such a mode of progression; and in the second place, owing to our knees, which suffered from the constant repetition of the shock against the superficial crust, each time we went down. In the mean time, day made its appearance, after having been announced by a sensible diminution of the temperature; and the sight of the summits becoming gilded one after the other, still further augmented the provoking annoyance caused by our detestable path. There is nothing more painful for a man who is conscious of possessing a certain degree of energy, than to feel himself yielding to the pressure of physical obstacles. I was conscious of my strength becoming exhausted at every step I made, and I was impatient at seeing Agassiz taking the lead. On no other occasion had I ever felt the slightest jealousy, because I should have had the conviction, that, by means of a little effort, I could have made up to him; but now, on the contrary, I was obliged to confess my weakness, and was therefore deeply mortified.

"We could have concluded from the aspect of the glacier, as seen from a distance, that no stream escaped from it, or at least that it was not visible externally, owing to the thickness of the snow. We encountered the last traces of water near the hut of the shepherd, in front of the gorge of the Oberaar, at a place where the snow was moist. Its temperature was at zero, and it proceeded, according to all appearance, from those springs which flow in summer on the polished flanks of the valley, and give rise to small marshes which here occupy the left flank. The cascade of the Trübtensee and the torrent of the Oberaar had disappeared, and their bed was only indicated by immense stalactites of ice, suspended on the rock. But what interested us most, was the outline and the form of the glacier itself. If the glacier had continued to move and to increase during the winter, it ought to have left traces of that movement; and as the snow accumulated at its extremity had fallen some months before, it would have been gathered together in a forward direction, or at least some swelling would have been perceptible, immediately in front of the terminal edge. In place of that, the snow presented a regular talus, such as is formed by the snow where it has been driven by the wind against the flanks of valleys.

"It was seven o'clock when we reached the edge of the glacier, and we had thus taken two hours to accomplish what in summer never occupied more than 40 minutes. I was dreadfully fatigued, and I had had so many and such repeated falls, that my knees were quite galled. I declared to Agassiz that I should proceed no further. If the plain had seemed so difficult, what would it not be when we should arrive upon the glacier? But notwithstanding my remonstrances, Agassiz was determined to persist; and he represented to me, that the crust could not fail to be softened when the sun had acted for some hours on its surface, and that our journey would then be much less laborious. These and other reasons induced me to attempt the ascent of the terminal edge of the glacier, which we found much less difficult than we had figured to ourselves. The snow had rendered the slope much more gentle than it is in summer; for not only was there no trace of the moraine to be discovered, but even the inequalities and the very considerable notches of the extremity of the glacier had completely disappeared. We saw no vestige of the stream. Having arrived on the surface of the glacier, we found here and there blocks whose tops emerged from beneath the snow; but their lower sides were alone visible, for the surfaces directed to the upper part of the glacier were invariably concealed by a covering of snow—a proof, that, in winter as in summer, the prevailing winds are from the west, parallel to the axis of the glacier. Having found the surface of the glacier more practicable than the plain, we decided on continuing our march, with the intention of retracing our steps afterwards. Our guides did not give us much encouragement, for they knew no better than we did the state of the glacier in winter. Our march was still very slow and arduous, as may be well supposed; but we did meet with portions where the snow was bearing. We then experienced extraordinary relief, and, in spite of fatigue, we ran like children upon the hardened surface, until the snow anew gave way under our feet, and again calmed our ardour. At about a third of the distance between the extremity of the glacier and the Hôtel des Neuchâtelois, we met with an enormous block supported on several pedestals, and covering a pretty considerable hollow, at the bottom of which we perceived pure ice. It was of importance for us to ascertain the state of the ice of the glacier. Having descended with due precaution into the hollow, we saw that the block covered the widened extremity of a *crevasse*, which penetrated to the right into the interior of the glacier, and whose walls exhibited that same magical reflection which is observed during summer; the azure appearing to us even more brilliant than usual, doubtless because this was the only point where we were able to see it. We were thus assured that the *crevasses* are not filled with w, but only covered by an arch more or less thick. Our guides assured us, that the water disappeared from those *crevasses* which contained it during summer, either by its flowing away or becoming frozen. Let it not be objected, that this is quite natural, and that it is useless to mention it. It frequently happens that, in summer, the mean temperature of several consecutive days is some degrees below zero (32° F.), without the water of the *crevasses* being frozen; there is only a pellicle of ice formed, which covers it during the night, and disappears during the day. A circumstance which inclines me to believe that the water they contain during the summer freezes rather than runs off, is, that, on the glaciers, we have on several occasions met with circumscribed portions of ice in the form of an ellipse, which had completely the appearance of congealed *baignoires.* The ice was full of air-bubbles, but its surface was perfectly smooth; it was extremely hard, and more brittle than usual. The bubbles were pretty uniformly distributed, and were nearly of an equal size, affecting in general an elongated and pyriform shape; but we did not remark any of those vertical laminæ of varied colour, to the study of which M. Agassiz means afterwards to devote particular attention. Subsequently we did not find any example of ice so full of bubbles, except on the last summit of the Jungfrau. It would be extremely difficult, in the present state of our knowledge, to point out the cause of these differences. Up to the present time we have only obtained very imperfect data regarding the modifications to which ordinary ice is subjected under the influence of atmospheric agents; and, as to the modifications which glaciers undergo in the interior of their mass, at different periods of the year, these are as yet entirely unknown.

"We were scarcely able to recognise our glacier of the Aar, so varied and animated in summer, under the uniform bed of snow. The great medial moraine itself was more or less effaced, and only formed a longitudinal ridge, whose flanks were much less inclined than in summer. We first of all reached the northern flank, and when we had passed over about a third of the glacier, we crossed to the southern flank, at the point where the moraine is considerably swollen. We there saw, to our great satisfaction, that our route improved more and more. The snow was much more compact, so that even when its external crust yielded, we did not sink very far. There was no longer any doubt of our arriving at the Abschwung. But another inconvenience came in the place of the difficulty of walking, and that was the intensity of the light. In proportion as the sun attained a greater elevation, its rays were reflected with such power by the millions of crystals of this vast snowy region, that the blue glasses with which we were provided became insufficient; and in order to remedy this, and to preserve the skin of our faces, we were obliged to envelope our heads in a double veil, under which we breathed as if we had been in the middle of summer. It was not without some astonishment that we here met with a small butterfly, which fluttered around us. It seemed to be perfectly at its ease, and was, according to M. Agassiz, the species named *Vanessa urticæ (La petite Tortue).*

" It was eleven o'clock when we arrived at the height of our old dwelling, and we were very much astonished at not being able to discover the Hôtel des Neuchâtelois. Was it possible to conceive that the immense block, which was seen from so great a distance in summer, and whose summit had so often reanimated the courage of our visitors, had been entirely interred in the snow? At last, after having sought for it on all sides of the moraine, we descried at some distance a swelling in the snowy ridge, and this proved to be our hotel. It was entirely covered by snow. On one side only we saw one of its walls uncovered for a space of some feet; but, in order to penetrate into the interior, it would have been necessary to clear away an enormous bed of snow, which would have occupied a great deal of time, and we therefore preferred reposing on the snow. Agassiz was in very high spirits, rejoiced to find himself, in such magnificent weather, in the midst of that sea of ice which he had made the scene of his observations. In truth, the spectacle which we had before us was of an unique character. It appeared to us, that we had never seen the air so transparent. The outlines of the mountains were delineated on the blue back-ground of the sky with a precision never witnessed in summer. All the peaks which bound the glacier were clothed with snow from their base to their summit; and the Finsteraarhorn alone was black as in summer, for its walls are too precipitous on the side next the glacier, to allow of the snow adhering to them. As to the glacier itself, it really did not exist for us at that time; for we had nothing else before us but an immense extent of very uniform snow, which wanted the magic charm given by moraines, as well as the crevices with their brilliant tints, the icy cascades, and the thousand rills of water with their harmonious murmur, all of which constitute the delights of the scene in summer. The two rods which we had introduced the preceding autumn into the holes that had been bored, and of which mention will afterwards be made, were elevated only a few feet above the surface of the glacier; but they had preserved their respective positions, and were both nearly vertical;—a proof that the superficial beds of the glacier had not since that period advanced in an unequal manner. We then ascended to the Abschwung, and saw that the snow had completely filled up the space between the rock and the *névé*. We estimated the thickness of the bed of snow at that place at 30 feet. At noon, we returned to the Hôtel des Neuchâtelois; and as I felt myself indisposed, I decided on returning with a guide. Agassiz remained for the purpose of making some observations on temperature. His object was to ascertain if the temperature of the snow was the same as at the Grimsel. With this view, he introduced into the snow, at a depth of 8 feet, the same thermometrograph, taking care to close the hole. After two hours, the instrument indicated—4°.5 (+ 25°.7 F.), the air being at + 1° (+ 33°.8 F.).

" The descent seemed to me as easy as the ascent had appeared difficult. We sank about half a foot; but the snow was firm, less powdery than in the morning, and slightly moist, which considerably facilitated our walking. It is not so much the thickness of the snow, as the inequality of the surface, that produces fatigue in these walks, and it does so by causing unexpected jerks, which are always annoying. I did not fail, whenever I met with an eminence which cast a shadow, to measure the temperature of the air, and I was much surprised to find, that the thermometer almost invariably remained about zero (32° F.) On one occasion only, I saw it ascend to + 1° (+33°.8 F.) M. Agassiz made the same observations, and obtained the same results. I ought to add, that these observations were made at a height of only one foot above the surface. It was impossible for us, from want of shade, to place the thermometers in a higher position, so that we could not determine, in an exact manner, the influence of the radiation of the snow. In the sun, the heat was excessive; and therefore we not only made no use of our cloaks, but, in order to be more at our ease, took off our coats and waistcoats. The necessity of keeping on our double veils was, in these circumstances, a real punishment. I attempted several times to remove them for a few moments, in spite of the advice of the guides, and afterwards I could not too much repent having done so, if it was to that cause that I had to attribute the miseries of the succeeding night.

"Agassiz rejoined us at the Hospice of the Grimsel about four o'clock. I had, in the mean time, prepared a bowl of punch, by means of the essence which we always took care to have in our possession; and I need hardly say, that this drink, invented expressly for icy regions, appeared to us a real luxury. When we were all seated round the table, in the small low apartment, which serves as a shoemaker's workshop, we experienced a lively satisfaction in recalling the most trifling events of the day; and, proud of our success, we formed a thousand projects for the future. It was then, among other plans, that we conceived, for the first time, the idea of attempting the ascent of the Jungfrau. Jacob had prepared the supper, which, like that of the preceding evening, consisted of rice soup, salt mutton, and chamois steaks. This last dish was not, I must allow, very juicy; but as it was of chamois, we were obliged to regard it as delicious.

"We soon went to bed, in order that we might be able to start at a very early hour in the morning; but we had scarcely reposed an hour or two, when I experienced the most violent pain in the face. My head seemed on fire, and I felt my cheeks swelling and my face cracking. In vain I sprinkled myself with cold water—I suffered the agony of a martyr. Agassiz [illegible] few minutes afterwards with a deep sigh. I am in great pain, he said ; my lips feel as if they were torn in pieces ; what can be done to assuage this suffering ? For a moment we thought of going out and immersing ourselves in the snow, but reflecting that such a remedy might produce serious consequences, we resolved to endure our misery till the morning. It was a terrible night for us. Towards the morning the pain gave us a little respite ; we reposed a few hours, and when we rose, we could not restrain our fits of laughter, on looking at each other. Are you aware that you have the appearance of a *cretin ?* said Agassiz. Have the goodness to ask for a looking glass for yourself, I replied. Our faces were coloured purple, and horribly disfigured ; I could scarcely open my eyes, so great was the swelling of my eyelids ; and Agassiz had his lower lip excessively swollen and pendant. Nevertheless, we decided on starting the same day. Our thermometers gave us the same results as on the preceding day ; that is to say, the thermometrograph, at a depth of five feet in the snow, indicated—3° ( + 26.6 F.), and the thermometer in the air stood at + 2° 5 (+ 36.5 F.), at eight o'clock in the morning."

It is apparent from this description, that the water which existed in the neighbourhood of the Grimsel was spring-water ; and what proves that the glacier of the Aar had not moved for a long time, is the fact that the snow was not gathered together at its extremity. We also observed the same continuity of the snow at the extremity of the glacier of Rosenlaui, and our guides assured us that the snow is never seen collected together by the ice in winter, as is the case with the gravel and the turf in summer, when glaciers are advancing. Every thing, therefore, concurred to convince me, that glaciers do not move in winter. The glacier of Rosenlaui, furnishes me with another proof of the same thing, and in the following manner : This glacier terminates at the edge of an abyss into which the torrent, escaping from its extremity, is precipitated in summer. Such a circumstance could not but be very favourable for my researches. I found, as I had foreseen, the terminal edge of the glacier exposed ; not a drop of water escaped from it, for, if there had been only a few drops, these must have found their way to the edge of the uncovered hollow. I am well aware that it may be urged, that perhaps this water loses itself in the interior of the rock, by flowing into a crevice or a cavern before reaching the extremity, and that, consequently, the absence of water at the terminal edge does not prove its total absence, any more than in the case of other glaciers. But supposing that it were so, the glacier ought at least to be moist at its bottom; for, assuredly, if the melting caused by the earth took place in winter in spite of the external temperature, it is not a glacier descending so low as that of Rosenlaui, and consequently surrounded by a warmer atmosphere than many others, which ought to be the exception to the general rule. Now, I convinced myself that the whole bottom of the glacier was frozen to its bed, and consequently that all movement was impossible. I believe myself therefore entitled to affirm as a thing demonstrated, *that glaciers are stationary in winter*, that the water which escapes from them is spring-water, and that it is not at all the result of the melting caused by terrestrial heat. For, as has been already remarked, if that melting really took place, it ought to occur in all glaciers, and not a single one ought to be dry.

I took advantage of this winter excursion to make another experiment relative to the formation of polished surfaces,—an experiment which, I believe, will not be devoid of interest for future researches. It is known that I attribute polished and striated surfaces to the action of ancient glaciers which formerly covered the localities; and I do so, because they are completely identical with those met with under existing glaciers, and along their flanks. At first a pretence was made of not assigning any importance to these polished surfaces, which were attributed to various causes; and M. de Charpentier himself, in his last work, seems to have allowed himself to be intimidated by the criticisms of opponents, for he says, "If there were no other facts to adduce in favour of the diluvian glaciers, but these marks of attrition and these furrows, the observation of M. Mousson (who attributes them to currents), might be well founded."* But since that time facts have spoken for themselves; and they have been more eloquent than the most beautiful theory could be. There is now no one who ventures to dispute the importance of a phenomenon so generally distributed; but some geologists, refusing to adopt the glacial theory, have pretended that the polished surfaces, on which glaciers of the present day repose, are not the result of the action of glaciers, but have been produced by a cause anterior to their existence. This opinion has nothing probable in it, because, in the Alps, the phenomenon is too intimately connected with glaciers to be regarded as independent of them ; but it would be of importance to have an experimental proof of this. As all the glaciers of the Bernese Oberland are at present in a state of increase, I conceived the idea of making an accurately determined mark at a place which I supposed would soon be invaded by the ice. For this purpose, I caused a corner of the glacier to be removed ; and it was by this means that I assured myself that the intermediate bed of mud and of gravel was so frozen as to be incorporated with the rock. We succeeded, however, in uncovering the surface of the rock ; and we then cut out in the polished rock, at a place whose position was accurately ascertained by means of fixed points, a triangle, having a base of about a foot, from which we removed the rock to the depth of half an inch, having rendered the surface as rough as possible. If, as I have no doubt, it be really the glacier which polishes the rock, this triangle ought to be repolished and striated within a certain number of years. We have no idea of the time required by a glacier to polish its bed, but it is probable that the duration of that time varies according to the weight of the masses, and according to the nature of the rock ; and, as the bed of the glacier of Rosenlaui consists of a black limestone sufficiently susceptible of being acted on, we may hope to obtain a result more speedily than if the rock had been gneiss or granite.

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* J. de Charpentier, Essai sur les Glaciers. p. 293.

I had hoped that the glacier would soon have invaded anew the place we had uncovered in the month of March ; but I was a little disappointed when I visited our glacier with Messrs Forbes and Heath in the month of August following, to see my triangle still exposed. The glacier had advanced, it is true, but on its left side, while the right flank had remained stationary. We cut out a second but smaller triangle under the ice itself, at a distance of 17 feet 5 inches from the first, and I hope to find both covered by the ice next summer.

Another objection which has been made to my theory is the following. If, as you say, the temperature of the glacier is constantly at 0° (32° F.) and under it, how can it be conceived that the water should remain liquid and penetrate into the interior of the mass? Will it not freeze by the simple contact of the ice? The action of the dilatation ought, therefore, to be confined to the surface, and ought not to produce any effect on the inferior beds of the glacier. This reasoning is undoubtedly just, but those who have made use of it as an objection here maintain a physical necessity which does not exist in nature, when they suppose that the infiltrated water, which acquires the temperature of the glacier by being introduced into it, must necessarily freeze when its temperature descends below zero. Because all ice has a temperature at least as low as 0°, it does not follow also, that water cannot exist below 0°. It will suffice to mention here, the experiments of Professor August,[*] which have demonstrated that water can be preserved in a liquid state, *in vacuo*, at − 12° ( + 5° F.), and even at − 13°5 R ( + 3°87 F.). According to that author, no shock, however violent, can produce congelation under these circumstances at a temperature of − 2°.5 R (+ 28°.6 F.) Now, may it not be the same with the water which penetrates into the interior of the glacier? But science ought not to rest satisfied either with possibilities or probabilities, when we have to do with a phenomenon accessible to our researches; and the question can only be determined in a definitive manner by direct experiment. I had, in other respects, so much the more interest in obtaining information on this subject, because, last summer, having reached no deeper than 25 feet in my boring attempts, the results which I obtained might, to a certain extent, have been attributed to the influence of the temperature of the external air. I therefore went a second time, in the month of August last, to the same glacier of the Aar on which I had lived during the preceding summer. On this occasion I remained more than a month including some more or less distant excursions (from the 8th August to the 10th September), having been accompanied by several friends who were naturalists, some of whom had been with me during the previous years. Messrs Forbes* of Edinburgh, and Heath of Cambridge, likewise spent nearly three weeks with me at the Hôtel des Neuchâtelois. Afterwards, my friend M. Escher de la Linth, took an active part in our labours. I passed 27 days altogether on the glacier, during which time I succeeded in sinking the piercer to a depth of 140 feet. As it is the first time that an attempt has been made to penetrate to a great depth into the interior of a glacier, it may not perhaps be uninteresting for my readers to be informed of the mode of procedure followed, and the results obtained.

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[*] Poggendorff's Annalen, vol. lii. p. 184; and extract in Bibliothèque Universelle de Genève, 1841, p. 191.

See also Professor Kries in Poggendorff's Annalen, 1841, No. 4; and in Edinburgh New Philosophical Journal, vol. xxxii. p. 198.—Edit.

The attempts which I made the previous year, had shewn me that a glacier could not be so easily pierced as one would at first suppose. I looked forward with pleasure to being able this year to observe the phenomena presented by the glacier at its contact with the rock. I communicated my new projects to M. Koehli, engineer at Bienne, and we were convinced that we should employ iron piercers like those used in the construction of artesian wells. M. Koehli was good enough to entrust me with his own piercer, which is 150 feet long. It was, he said, the greatest depth which I could reach by piercing with the hand; while a larger piercer would have required considerable scaffolding, and an outlay of money which would greatly have exceeded my pecuniary resources.

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* Notwithstanding the painful disputes which afterwards arose respecting certain phenomena in the structure of glaciers, whose discovery was claimed by Professor Forbes, I am far from regretting our residence together on the glacier; and if any thing could console me for the vexation I experienced on this subject, it is the thought that the visit of that gentleman to the Hôtel des Neuchâtelois, has contributed to diffuse more widely in England and Scotland the knowledge of the mechanism of glaciers.

As, however, the thickness of glaciers is generally estimated at from 80 to 100 feet, I hoped to reach the bottom of that of the Aar with this boring apparatus. In order not to be stopped by unforeseen difficulties, I took with me M. Koehli's foreman, who was entrusted with the direction of the boring operations. The piercer was composed of ten bars, an inch in diameter and fifteen feet long; the cutting portion of the various *fleurets* was three inches, three inches and a half, five inches, and six inches in diameter. All the bars, as well as the other instruments connected with the piercer, such as the keys, *cuillers*, ropes, &c., were carried by men from Meyringen to the Hospice, and thence to our hut on the glacier, that is to say, to a distance of ten leagues from Berne. I established myself, along with my companions, under the same block which had sheltered us the preceding year, and which is now known to the scientific world by the name of the *Hôtel des Neuchâtelois*. It is a large block of mica-slate, forming a part of the moraine which descends from the flanks of the Schreckhorn, at a league higher up. Its length is 41 feet, its breadth 30 feet, and its height 19 feet. One of its angles projects in the form of a roof on the south-west side. It was this place which we again chose on this occasion as a shelter. I had sent some days previously two of my guides to the glacier to prepare our abode. They found the walls which had been constructed the previous year completely dislocated by the movement of the glacier. The space, however, which they contained, would not have been sufficiently large for our encampment of this year. The hut was therefore rebuilt, and arranged in such a manner as to shelter eight persons. The kitchen was prepared in front of the entrance to the hut, and the store-room at the side, under another block. The guides constructed for themselves a second hut on the left side of the glacier, at the distance of half a league. It was not, like our own one, built on the ice, and, in this respect, it was more solid and less precarious; but ours had the advantage of being situated in the middle of the glacier, at the most favourable point for our observations.

As soon as we were fairly settled in our hut, I commenced the [illegible] operations. In the previous year I had tried two kinds of instruments, a piercer with two teeth, and one with a *fleuret* having four teeth; but I soon perceived that the latter instrument had the disadvantage of grinding the ice too violently, which often rendered it extremely difficult to draw it out. This year I had only brought simple chisels of different diameters, and another instrument having the form of an inverted funnel, with circular and angular edges, which our miners call a *couronne*, and of which they make use in piercing very soft substances. At first, and to a depth of 40 feet, this instrument worked very well, but afterwards it became necessary to give it up; for, although its edges were very blunt, yet the weight of the rods becoming always greater, caused it to be too firmly fixed in the ice, and rendered its withdrawal a matter of infinite difficulty. We therefore exchanged it for simple chisels.

I had remarked that the piercer acted better when the hole was filled with water than when it was dry; and, accordingly, I took care to place the bore in communication with one of the numerous rills of water which flowed over the glacier. This had, moreover, the advantage of enabling the borers to dispense with the necessity of emptying the hole every minute; for all the splinters of ice which the piercer detached at the bottom of the hole, came up of themselves to the surface, merely owing to their smaller specific gravity, and they were carried away by the current. But this proceeding had likewise its inconvenience; for, as I wished to make daily observations on the temperature of the interior of the glacier, I was obliged to cause the bore to be emptied every evening, which was a pretty long operation.

In this manner, a depth of 70 feet was reached. The piercer then began to become too heavy, and the chief borer proposed to me to construct a *trépied* above the hole, and to fix to it a pulley, on which a rope was to be passed, to which the piercer should be attached. He hoped by this means to bore more rapidly, and with less trouble. The operations were on this account suspended for several days, during which my people went to the valley of Hasli, to procure young fir-trees for the scaffolding. When the *trépied* was prepared, hey transported it to the glacier. It was now intended to proceed with renewed ardour; but what was our surprise, when we were about to introduce the piercer into the hole, to find that it would no longer enter. We then remarked that the hole had become contracted by half an inch, and we had no other course left but to recommence anew. We required three days to reach the depth of 70 feet, at which we had left the bore. For my own part, instead of regretting this loss of time, I rejoiced at the circumstance, for I had thus obtained the most manifest proof of the dilatation of the glacier to a considerable depth. The contraction was not merely superficial, but extended as far as the bore did; and it could not be the result of the water congealed along its walls, for care had been taken to remove the little streamlets, and I had caused the few inches of water which accumulated at the bottom of the hole, in consequence of transudation, to be taken out every evening.

One day the workmen felt the piercer escape from their hands, and fall down two feet. They were then at a depth of 110 feet. It was evidently an internal cavity which had been encountered, and a certain quantity of air-bubbles were soon seen arriving at the surface, after having traversed the column of water which filled the bore. Unfortunately I was not near at the time, and the bubbles of air, whose nature it would have been interesting to ascertain, could not be collected. But the fact is of itself not the less important, because it furnishes us with the proof that there are cavities in the interior of glaciers at very considerable depths, and in places where the surface does not exhibit any trace of a large and deep *crevasse*. The splinters which were detached by the piercer, and which ascended to the surface, had the same appearance and the same hardness as if they had been detached from the wall of a *crevasse*. In two other cases, bubbles of air rose from the bore, without, however, the piercer descending suddenly. I have also seen them frequently ascend to the surface of hollows in the ice (*baignoires*), filled with water.

I was provided with three of Bunten's thermometrographs, which I placed every evening in the following manner: one in the great bore; a second in the hole less deep, generally at a depth of 15 feet; and a third in the air. I took care to close the openings of the two holes, so as completely to prevent the external air from influencing the temperature of the air contained in those holes. During the four weeks that these observations were continued, the temperature of the air at night never descended below – 6°.5 (+ 22°.1 F.); the minimum has even been + 2° (35°.6 F.) during the night, on one occasion. Of course, the streamlets of water on the surface of the glacier then continued to flow, and did so even when the temperature was at + 1° (33°.8 F.), or even at 0 (32° F.). In the interior of the glacier, the thermometrograph sometimes indicated 0 (32° F.), and then the bore remained moist, and water was accumulated at the bottom; sometimes it descended to – 0°.3 (31°.46 F.), and even to – 0°.5 (31°.1 F.); the sheath of the thermometrograph, or the cord which held it, was, in such cases, sometimes frozen to the bottom of the bore; and I was obliged to disengage it by pouring boiling water down along the cord. On the morning of the 15th of August, the thermometrograph, which had been during the night at a depth of 60 feet in the glacier, was at 0 (32 F.); the external air had been at – 3° (+ 26°.6 F.). On the 16th it was at the same, while the air had been at + 1° (33°.8 F.); on the 17th the thermometrograph indicated – 0°.5 (31°.1 F.), and the temperature of the air had descended to – 2° (+ 28°.4 F.). I quote these three observations because they were made at the same depth, during an interruption of the boring. I intend publishing afterwards all the observations made at different depths. I shall merely now add, that, on the night between 31st August and 1st September, I placed two thermometrographs in the same hole, the one at a depth of 15 feet, and the other at 125 feet, and that I found both at 0 (32° F.) in the morning. The result was the same during the day of the 1st September, and during the nights of the 1st and 2d and 2d and 3d September, at the depths of 15 and 100 feet; while the maximum of the external air during the day was + 8° (46°.4 F.), and the minimum of the two nights was + 0°.7 (+ 33°.26 F.). During the day of the 3d September, I found the temperature at 0 (32° F.), at 15 and at 125 feet in the same hole, the external air being at + 3° (37°.4 F.).

I had observed, that whenever the cold of the night was not very great, water was infiltrated into the bores; and on several occasions I found in the morning my thermometrographs submerged, although I had taken care to empty the bore completely the preceding evening, and although the introduction of water from above was prevented by little drains, and by the manner in which the aperture was closed. I saw at once the importance of this fact for the theory of dilatation, and I proceeded carefully to examine this accumulation of water. The following is the method I adopted. After having thoroughly emptied the bores, I measured from time to time the quantity of water that had been accumulated, by plunging into the hole a sounding line, which I kept stretched in such a manner as to rub against the sides of the hole as little as possible. The length which I found to be moist when I withdrew it, gave me the depth of the accumulated water. The following observations were made simultaneously in two bores, of which the one had a depth of 30 feet, and the other of from 120 to 140 feet. The first of these holes, of equal diameter throughout, had been bored with a *fleuret* of 3½ inches in diameter; the large hole, on the contrary, went on decreasing in diameter from above downwards, having been bored to a depth of 92 feet with a *fleuret* 6 inches in diameter, from 92 to 110 feet with a *fleuret* of 5 inches, and beyond 110 feet with a *fleuret* of 3½ inches.

*Quantities of water accumulated in the small and large bores.*

| DATE. | SMALL BORE at a depth of 30 feet. | | LARGE BORE at first at 120 feet, and afterwards at increasing depths to 140 feet. | | Temperature of the air. | |
|---|---|---|---|---|---|---|
| | | | | | Minimum. | Maximum. |
| | ft. | in. | ft. | in. | | |
| Night of Aug. 31 —Sept. 1....... | 0 | 6 | 6 | 0 | +0°.3 (32°.54 F.) | ............... |
| Day of Sept. 1 ... | 9 | 10 | 29 | 6 | ............... | +8°.7 (47°.66 F.) |
| Night of 1st—2d | 15* | 0 | 12 | 0 | +0°.7 (33°.26 F.) | ............... |
| Day of 2d......... | 2 | 6 | 10 | 0 | ............... | +2°.5 (36°.5 F.) |
| Night of 2d—3d.. | 0 | 6 | 3 | 6 | +0°.7 (33°.26 F.) | ............... |
| Day of 3d......... | 2 | 8 | 20 | 0 | ......... ..... | +3°.0 (37°.4 F.) |
| Night of 3d—4th | 0 | 3 | 3 | 0 | +1°.0 (33°.8 F.) | ............ .. |
| Day of 4th......... | 3 | 4 | 18 | 0 | ............... | +3°.6 (38°.48 F.) |
| Night of 4th—5th | 1 | 8 | 4 | 0 | −4°.0 (+24°.8 F.) | ............... |

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* During this night the small bore remained open, and some water was introduced from the surface, whereas the large bore was closed.

It results from this table, that the quantity of water accumulated in the two bores was not only different, but was proportional to the surfaces of the bores; whence we conclude that it is by no means introduced from the opening at the top, but that it is exuded by the sides. If it were otherwise, the small bore would not on every occasion have contained a quantity of water so small compared with that accumulated at the same time in the large bore. The contrary would even have taken place, because the small bore was narrower than the large. The fact of the quantity of water being unequal on the different days is also not without its importance, for it proves to us that no fissure or canal existed which could place the bores in communication with any reservoirs of water; for, in that case, the accumulation of water would have been proportional to the interval of time comprised between the observations. There is only one conclusion to be drawn, which is, that the glacier imbibes water in unequal proportions at different hours of the day, at different external temperatures, and according as the air is dry or humid; or, in other words, that the glacier should be regarded as a spongy body of ice, which has imbibed a larger or smaller quantity of the water that circulates in its interior.

It is true, that, during the five days when the above observations were made, the temperature fell only once below 0 (32° F.) during the night, and that it remained most frequently above 0. We have had other nights during which the minimum has been at + 2° (35°.6 F.). We must not, however, proceed too rapidly to draw from this inferences adverse to the theory of dilatation, and above all we must not thence conclude that the glacier only imbibes as much water as there is at the surface. The state of the atmosphere no doubt influences the quantity of water which circulates in the glacier, but this influence is not such as to prevent water existing in the interior of the mass even when the temperature falls temporarily under 0 (32° F.). During the night of the 4th and 5th September, the temperature of the air fell to − 4° (+ 24.8 F.); on the evening of the 4th it snowed, and on the morning of the 5th the glacier was covered with six inches of snow, and nevertheless we have seen that, notwithstanding the cold, the small bore collected that night 1 foot 8 inches, and the large bore 4 feet of water. The snow was perfectly dry, and did not begin to melt until 6 o'clock P.M.

If these facts were not sufficient to prove that water circulates in a liquid state in the interior of the glacier, I could cite others not less conclusive. I have often seen water exuding along the smooth walls of the *crevasses;* and it sufficed to wipe the moist place, to see a little drop of water rise to the spot over which I had passed my handkerchief. Lastly, it will afterwards be seen, that, on my descent to the bottom of a crevasse 120 feet deep, I found the walls of ice bristled in a multitude of places with small icy stalactites, four or five inches long, which evidently proceeded from the drops of water exuding at all these points.

It appears to me to be demonstrated, that water is diffused in four ways in a glacier :—

1. By hollows open at the surface, to which numerous rills of water resulting from the melting of the superficial ice contribute their supply. These hollows, opening widely to the surface, are *crevasses*, apertures of cascades, *baignoires*, and vertical tubes, at the bottom of which there are small fragments of rock. It is not necessary to give any other proof of this mode of diffusion than the following fact: Whenever the night has been very cold, the surface of these pools of water is frozen, but the water beneath is lowered by two or three inches, and covered by a second and thinner crust of ice reposing immediately on the water; sometimes, indeed, the water of cavities of small extent has entirely disappeared, although the plates of horizontal ice which cover these hollows indicate that there must have been water to the depth of a foot and even more. I have never, however, seen large bath-shaped hollows (*baignoires*), full of water, lower their level more than a few inches in the course of a night. I ought to add, that I have never met with a *crevasse* which went to the bottom, except at the extremity of the glacier.

2. By internal canals which circulate in the whole mass, like arteries of very various sizes, and which are seen terminating here and there on the walls of the *crevasses* and on the terminal edge of the glacier, where they sometimes form little

cascades and even jets similar to that of a fountain, as was well seen last year in the glacier of the Rhone; but most frequently their opening is very small, and is hardly a line in diameter; they then give rise to small stalactites, of which I shall afterwards speak.

3. By capillary fissures, which divide the whole mass into a quantity of angular fragments of various sizes. We may assure ourselves of this by pouring, as I have done, coloured liquids into cavities hollowed out at the surface of the glacier.

4. Lastly, it seems to me probable that the vertical bands of blue compact ice, alternating with bands of white porous ice, and which I shall discuss immediately, maintain a continual infiltration of water in the mass of the glacier wherever this structure is observable.

The demonstration of the presence of water in the entire mass of the glacier, at all depths, is the most important of all the facts obtained during my residence last year on the glacier of the Aar; and I am so much the more interested in bringing this clearly out, because it is upon the absence of water at great depths, that many authors, and especially Mr Hopkins, in his essay entitled Theoretical Investigations of the Motion of Glaciers, found, in a great measure, their reasoning against the glacial theory. Without pretending that all the difficulties can be removed by the discovery of this single fact, it will at least be conceded that it has enabled us to advance a step farther towards the solution of the problem.

I now come to a no less important fact, that of the lamellar structure of glaciers. This remarkable phenomenon, which Professor Forbes has described extremely well in the Edinburgh New Philosophical Journal,* but of which he has erroneously claimed the discovery, and has assigned to it a generality that the facts he has himself observed did not at all justify, was for the first time observed in 1838 by Professor Guyot of Neuchâtel, on the glacier of the Gries, at a height of 7500 feet; and that naturalist made it the subject of a highly interesting communication to the Geological Society of France, assembled that same year at Porrentruy. I had myself, likewise, remarked this phenomenon on the Glacier des Bois, during an excursion which I made at the same period, and afterwards on the glacier of the Aar; but it presented itself nowhere with that striking distinctness we witnessed last year and it is on this account that, like several other facts whic continued observations could alone elucidate, it is only mentioned in a vague manner in my *Etudes sur les Glaciers.*

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* For January 1842, vol. xxxii. p. 84.

During the months of August and September 1841, thi phenomenon was so well developed in the glacier of the Aa that it could not fail to strike every observer, especially in th environs of the Hôtel des Neuchâtelois, at a height of 7500 fee and at a distance of two leagues from the extremity of th glacier. I shall now endeavour to describe it, and to give a account of the researches carried on with a view to ascertai its extent and modifications.

It is known that in general the ice of glaciers differ from ordinary ice, by having a network of capillary fissure which seems to penetrate its whole mass, and is the resul of the transformation of the névé into ice. But this ice i nevertheless, of great compactness, and though rough at it surface where it is not covered by a moraine, that appearance is only superficial; and it suffices to remove the externa crust, to find the ice as compact as under the moraine, an at every place where it is sheltered from evaporation. I then, we examine this ice, we find that it is by no means uniform, but, on the contrary, that it is composed of laminæ o vertical bands of variable breadth, being generally from ½ to 1 2, and 3 lines, but sometimes also from 1 to 2 and even from to 5 inches broad. One set have a bluish tint, and a very compact and homogeneous texture; and the others have a whit colour, their ice being less hard, and having a snowy appearance, in consequence of the quantity of bubbles of air wit which it abounds. The whole may be compared to a mass o glass, composed alternately of dull bands with air-bubbles, an bands which are perfectly transparent. This particular arrangement of blue bands alternating with white ones, is particularly apparent in the walls of the *crevasses.* The bands pas from one wall to the other, and can be traced for spaces of hundreds of yards, always preserving a perfect parallelism. They are to be seen descending in this manner into the mass of the glacier, as far as the eye can reach into the *crevasses*, sometimes to a depth of ten and fifteen metres (yards). Their direction is generally parallel to the axis of the glacier; but they are not always rectilinear; for I have seen on the glacier of Aletsch and elsewhere, contortions and dislocations of different kinds, and M. Guyot remarked the same thing in the Gries glacier. Although the observations made at the beginning of our residence on the glacier of the Aar induced me to believe that this lamellar structure extended to a large portion of the mass of the glacier, I was nevertheless desirous to assure myself, in a direct manner, that it did so, and with this view, I descended into one of those hollows or pits in the glacier, where most of the streams which trace a serpentine course on its surface are swallowed up. I was thus enabled to follow the blue and white bands as far as the accumulation of water at the bottom of the hole permitted me to descend, that is to say, to a depth of 120 feet.

The following is a short account of the descent, which my travelling companions afterwards denominated my *descente aux infers*:—

It was towards the termination of our residence on the glacier; we had finished our boring, and were preparing to depart, when, while discussing, according to custom, the phenomena we had observed, one of the party remarked that it would perhaps be easy to descend without danger into some one of the pits of the glacier, and that perhaps some unexpected appearances might thus be observed. All were of this opinion, and without delay we commenced looking for a pit suited to the purpose. These pits, as I have remarked in my *Etudes sur les Glaciers*, are probably old crevasses, which a small stream of water has prevented from being completely closed; so that instead of being of an elongated form, they are, on the contrary, for the most part circular, and the rivulet, far from contracting them, tends to enlarge them more and more, especially when it is considerable. We found at some distance from our hut one of these pits, which seemed well adapted for our object; it had an opening of eight feet in diameter, and appeared to penetrate vertically to a great depth. I resolved to descend into it; but to accomplish this it was necessary to begin by turning off the stream by cutting another bed for it in the ice. We set all hands to work, and when the new bed was formed, I sent my men to procure the *trepied*, which had been used for the boring operations, and placed it over the pit. A board on which I was to sit was fixed at the end of the rope, and I was secured to that rope by a strap, which passed under my arms, so that my hands were left free. In order to protect me from the water, which we were not able to turn off completely, the guides covered my shoulders with the skin of a goat, and placed on my head a cap made of a marmot's skin. Thus accoutred I descended, provided with a hammer and a staff. My friend Escher was to direct the descent, and for this purpose he lay down on his face, with his ear hanging over the side, in order the better to hear my directions. It was agreed that so long as I did not ask to come up, I should be allowed to descend as far as the distance at which M. Escher could distinctly hear my voice. I reached a depth of 80 feet without encountering any obstacle, observing attentively the lamellar structure of the glacier and the small stalactites of ice of which I have already spoken, and which were attached on all sides to the walls of the pit. These stalactites were from 2 to 5 or 6 inches long, and only a few lines in diameter; and they were bent like hooks fixed in the walls. It was evident that they were produced by an exudation from the walls of the pit; for if they had resulted from the water falling from the surface of the glacier, they would not have been so uniform nor so equally distributed over the whole surface of the sides. Those which were really derived from the cascade of water from above were much larger, were more closely united to the wall of ice, and were, moreover, limited to one of the surfaces of the passage. The bands of blue ice became perceptibly broader as I descended; they were less sharply marked than above, and the remainder of the mass, of an inferior degree of whiteness, was less distinctly contrasted with the intermediate deeper coloured laminæ. At a depth of about 80 feet I encountered a ridge of ice which divided the pit into two compartments, and I endeavoured to enter the widest; but I could not penetrate more than 5 or 6 feet, because the passage became divided into several narrow canals. I caused myself to be raised up, and managing so as to make the rope deviate from the vertical line, I got into the other compartment. I had observed in descending, that there was water at the bottom of the pit, but I supposed it to be at a very great depth; and as my attention was especially directed to the vertical bands, which I continued to trace, thanks to the light reflected by the brilliant walls of the ice, I was very much astonished when I suddenly felt that my feet were immersed in water. I immediately directed myself to be drawn up, but the order was misunderstood, and in place of ascending, I found that I was descending. I then uttered a cry of distress, which was heard, and I was raised up before being obliged to have recourse to swimming. It seemed to me as if I had never in my life encountered water so cold. Fragments of ice floated on its surface, which no doubt were broken portions of stalactites. The walls of the pit were rough to the touch, and this was doubtless caused by the capillary fissures.

I should have wished much to remain a longer time to examine the details of the structure of the ice, and to enjoy the unique spectacle presented by the blue of the sky, as seen from the bottom of the abyss; but the cold obliged me to ascend as soon as possible. When I reached the surface, my friends told me of their anxiety for my safety when they heard my cries, and that they had experienced the greatest possible difficulty in drawing me up the pit, although they were eight in number. I had, however, reflected but little on the danger of my position. Perhaps, if I had known it previously, I would not have exposed myself to it; for, if one of the large pointed flakes of ice lining the walls of the cavity had been detached by the rubbing of the rope, and had struck me in its descent, my destruction would have been certain. I would, therefore, advise no one to repeat the experiment, unless it should be for some important scientific purpose.

I was thus enabled to follow out the lamellar structure of the glacier, not only as far as the first interruption of the canal, that is to say, to a depth of about 80 feet, but even, although less distinctly, to the bottom of the hole. I therefore consider myself entitled to conclude that the laminæ traverse completely the glacier properly so called, becoming more and more blended in its mass, whereas they exhibit very different phenomena in the higher regions, as I shall afterwards notice. Everything leads us to believe that the phenomenon of the lamellar structure is connected with the infiltration of the water into the mass of the glacier; and as we have seen, by the experiments mentioned above, that water exists in a liquid state as far as we have yet been able to penetrate, and that, on the other hand, the lamellar structure extends also, according to all appearance, through the entire mass, it must be by observing in a continued manner the mode of the infiltration of the water in the ice, especially at places where the lamellar structure begins to present itself, that is to say, at the limit between the névé and the glacier, that we can hope to attain the explanation of this important phenomenon. I propose to prosecute these observations during the sojourn I expect again to make this year on the glacier of the Aar. At the same time I beg those naturalists and natural philosophers who may visit other glaciers, to direct their attention to this subject, being persuaded that they will deduce important results, not only for the study of glaciers, but for physics in general.

I ought here to mention an experiment which I made with the view of ascertaining the modifications to which the different bands of this ribboned ice are subjected by the influence of the air. At a spot where the lamellar structure was very distinct, I uncovered a space about a foot square under the moraine, taking care to clean the surface well. We observed attentively the action which the air exercised on this surface newly exposed. At first the blue bands began to grow pale insensibly; fissures seemed to be formed in their interior; but I rather think that they merely became visible by the displacement of the air contained in the mass, and which is often n bursting out in small bubbles at the surface. At the end of half an hour the blue bands could only be vaguely distinguished from the white ones; the surface had assumed a very rough aspect; some hours afterwards it had even become entirely porous to the depth of about an inch; the lumps which were formed had become mobile, without, however, being disaggregated; the differences of tint had then completely disappeared, and the ice appeared composed of unequal but homogeneous fragments. Nevertheless, by moistening largely with water this unequal surface, the ribboned arrangement and the tints of defined colour reappeared immediately, and anew gave to the ice the lamellar aspect which it had originally. When the influence of the atmosphere was prolonged for a considerable period, the white bands at last became completely disaggregated to the depth of about a foot, and were then transformed into small grains perfectly similar to those of the *névé*, whereas the blue bands only appeared between these beds of grains as projecting edges soiled by the dust which lodges on glaciers. It is in the white bands, which are originally hard, but have become granular in consequence of the external temperature and the melting, that the small rills of water collect, which wind over the surface of the glacier. But if the glacier should be inundated by a violent rain, on the following day its blue and white tints at the surface contrast anew with a striking intensity. This, at least, was what we saw take place last year, and it remains to be ascertained to what extent this reiterated phenomenon of disaggregation and of congelation is reproduced at different seasons, and under influences of various kinds, and what are the circumstances which render the whole more or less apparent at the surface of the glacier. It is probable, according to what has just been stated, that the white bands are a *névé* impregnated with water and congealed, whereas the blue bands appear to me to be bands of ice of liquid water transformed directly into homogeneous ice.* It is difficult to determine the cause which determines this linear and continuous disposition of these vertical laminæ, but it seems to me that the first impulsion of this structure is caused by a molecular movement of the névé soaked with water, and purified by the running of the water according to the line of the greatest slope. If this were once established, it would not be at all extraordinary, that, in the lower part of the glacier, and notwithstanding the changes its course might undergo, this original disposition should be maintained by the direction which it would impress on the water that infiltrates into its mass by penetrating from the surface to the interior. It is at least only in this way that I can explain the more or less considerable obliquity of the laminæ on the outside of the moraines, and at the edges of glaciers where the largest quantity of water circulates ; and it is only by this means that we can explain the sinuosities and the contours that they form, when the masses are displaced from their natural direction during the long course of their descent from the high regions towards the extremity of the glaciers. It is well worthy of remark, that the stratified fields of snow of the highest slopes of the Alps do not present any trace of this lamellar structure, and that in the *névé*, where it passes into glacier,* this structure is superficial, or only penetrates a few feet into the mass ; it is only in the main course of the glacier that it seems to me to be well developed and to penetrate very deeply ; at the lower extremity it gradually disappears. It still remains to be determined if traces of it exist at the inferior surface of the mass of the glacier.

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* It would be interesting to ascertain if the same thing takes place in ice houses, containing piled up ice which has been sprinkled with water.

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* I only wish to describe in this article the facts relative to glaciers properly so called, because these are more immediately connected with the theory of progression by infiltration. It would have led me too far to have detailed the new observations which have been made on the fields of snow of the highest regions, and on the various phenomena presented by the *névé*. M. Desor and I have endeavoured to delineate the limits of these three regions (the fields of snow, the *névé*, and the glaciers), on a map of the glaciers of the Bernese Oberland, which accompanies the narrative of our ascent of the Jungfrau, by M. Vogt. (Jent and Gassmann, Soleure, 1842.)

The movements of glaciers, and more especially those of the lower glacier of the Aar, have for several years been the object of my continued attention. In my *Etudes* (p. 150), I have stated the progress of this glacier from 1827 to 1840, which, after taking into account the corrections lately made by M. Hugi, as to the distance of the hut from the *Abschwung*, would amount to about 220 feet per annum. In August 1840, the Hôtel des Neuchâtelois was 2457 feet from the angle of the rock called the *Abschwung*, which separates the branch of the Finsteraar from that of the Lauteraar. I took care to inscribe this measurement on one of the walls of the block. The following year, on arriving at the Hôtel des Neuchâtelois, my first proceeding was to measure the distance anew, and I then found it to be 2623 feet. The block had therefore advanced 166 feet. This new measurement was likewise inscribed on the block, and I am sure that on again visiting the glacier of the Aar this year, I shall find that the block has advanced about an equal distance. This is what any traveller may ascertain who happens to be at the glacier before me. If it be kept in mind that this annual progression takes place at a point where the glacier is very little inclined, inasmuch as its slope is scarcely 3°, and that at the same time the upper masses, and especially the branch of the Lauteraar, are very uniform, and do not present any trace of a rapid movement, it will be understood that a sliding movement, such as is supposed by many authors, is not at all probable. It suffices, moreover, to have seen the localities, in order to be convinced that at that point the glacier could never have advanced otherwise than by a slow and continuous movement, caused by infiltration and the daily congelation of the water which penetrates the whole mass.

These measurements ought further to establish, in another manner, the evidence of the movement by dilatation. It has been reasonably said: If it be the infiltrated water which, by becoming dilated, occasions the progression, it would not only be necessary that the whole mass should advance, but that the distance between two given points on the glacier itself should also be augmented; the same thing ought to occur here as in a mass of dough, which is made to ferment, and all of whose parts become dilated. Now, this is precisely what takes place in glaciers, as I have been enabled to infer from the distance which separates the block under which we lived, from another large block situated lower down, and known by the name of Hugi's Hut (*Cabane de Hugi*). The two huts are placed on the summit of the great medial moraine, consequently in the middle of the glacier, and under precisely similar conditions. When I established myself on the glacier in the month of August 1840, the distance from the *Hôtel des Neuchâtelois* to the *Cabane Hugi* was 1890 feet; in the month of August 1841, the distance amounted to upwards of 2000 feet; hence it had increased by about 110 feet.* On the other hand, we have just seen that the distance of the *Hôtel des Neuchâtelois* from the *Abschwung*, had been augmented during the same year by 166 feet in 2457; whence it results, that the progression was about the same in the two spaces (166 feet in 2457, and 110 in 1890). Last year, before quitting the glacier, I took, along with my friend M. Escher, other measurements, which will serve, I hope, to confirm these results. We ranged in a line, at three different places, a series of stakes, whose positions corresponded exactly with fixed points on both sides of the glacier. As these stakes are all inserted to a depth of 10 feet, I hope to find them still erect this year. It is probable that they will no longer be in line, and those which have advanced the farthest in the direction of the slope of the glacier, will indicate the portions of the glacier which have progressed most rapidly.

But the glacier was not only dilated in the direction of its slope, it was also considerably swollen, especially in the branch of the Finsteraar, at some distance from our hut. This swelling was so visible, that on approaching it for the first time, in the month of August last, every one was struck, the guides as well as ourselves; and I believe that in calculating the swelling at about 10 feet, we are far under the truth. It was quite close to the place where, in the preceding year, I had inserted two poles in the bores, the one to a depth of 9 feet, and the other to a depth of 20, for the purpose of ascertaining if the glacier really rejected foreign bodies. I burned with impatience to discover what had become of my two poles. We found both erect; but having measured them, I ascertained that they had been elevated 7 feet; so that the longest was only sunk to a depth of 13 feet, and the other would scarcely stand in the hole, which was not more than a foot and a half deep. The contact of the wood had also sensibly enlarged the two holes, more particularly the second.

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* I cannot give these numbers exactly to a foot, owing to the deficiency of our means of measurement; but if there be an error, it cannot, at all events, be considerable, as we measured the distances with a rod.

At first sight the matter seems quite simple, and it might be said that it was the level of the glacier which had been lowered by the melting. But we must not forget that these poles were planted on the portion of the glacier which had become most swollen since the preceding year, and this necessarily complicates the question, for how can we conceive at once a swelling of 10 feet and a lowering of 7 feet? Ablation had, nevertheless, actually taken place; and if, notwithstanding this ablation, the glacier became swollen as it progressed, that could only be because the augmentation of volume, resulting from the congelation of the water accumulated in the interior of its mass, had been more considerable than the volume of ice removed at the surface by the melting and the evaporation. A similar occurrence ought to present itself whenever the summer is rainy, as in 1841, for then the evaporation is less considerable, while an enormous mass of water is infiltrated daily into the glacier. We found, in fact, all the glaciers of the Oberland in a state of increase; and in all places they threatened to invade the pastures.

The experiment which I have now mentioned was also made by M. Escher de la Linth on the glacier of Aletsch; and the results he obtained were still more striking as to the rapidity of the disappearance of the ice. M. Escher, with the view of verifying the opinion I had expressed on the metamorphoses of glaciers, proceeded in the course of the month of June last year to the glacier of Aletsch, taking with him an ample provision of stakes, which he had caused to be prepared in the Valais. He described, by means of these stakes, planted at equal distances from one another, a triangle placed in relation to several fixed points at the sides; and he hoped by this means to arrive at a knowledge of the movements of the glacier, not only in the direction of the longitudinal, but also in that of the transversal axis. In order that the winds and storms might not knock over his stakes, he sank all of them to a depth of four feet. What was his surprise when, having gone about the middle of August to visit his triangle, he found that most of the stakes were down, and that those which were still erect did not penetrate more than half-a-foot into the ice! Twelve days afterwards (the 28th August), when I ascended the same glacier on my route to the Jungfrau, I found only one of the stakes erect. Thus two months were sufficient to remove a bed of 3½ feet in thickness from the glacier of Aletsch; and notwithstanding, M. Escher assures us that no difference in the level of the surface was perceptible.

These experiments were so important that I resolved to continue them. Foreseeing that, in the following year, the bore which I had pierced to a depth of 140 feet would be closed, or at least so much contracted as to be no longer of any use to me, I determined to employ it for a similar experiment. I therefore took fourteen wooden cylinders, each a foot long, and having a somewhat smaller diameter than that of the bore. I numbered them carefully, and having sunk No. 1 to the bottom of the hole, I covered it with a bed of gravel of 9 feet. I then introduced cylinder No. 2, which I covered in the same manner with a column of gravel of 9 feet, and so on in succession; so that in this manner the entire bore contained all the fourteen cylinders, separated from one another by columns of gravel 9 feet long. The fourteenth was 1½ foot under the surface of the ice on the 6th September 1841. As the position of the hole was exactly determined and easy to find, we can easily ascertain what has been the amount of ablation in a given time. Supposing that this ablation is on an average only 5 feet in the year, and that the glacier continues to advance in the same proportion—that is to say, about 200 feet annually—the cylinder No. 1 ought to reach the surface in about 28 years, and at a distance of 5600 feet from the place where it was inserted. In this calculation the inequality of the rapidity of the beds is certainly not taken into account, and which, according to the most recent researches, must be less considerable than I at first supposed.

The second bore, having a depth of 30 feet, was employed for an experiment from which I expect still more important results. We have seen, from the general view I have already given of the temperature of the interior of the glacier, that this temperature varied but little during the whole period of our residence on the glacier. It was important to ascertain if this was also the case during the colds of winter, when the glacier is covered with a thick bed of snow. I therefore resolved, with this view, to make use of my three minimum thermometers. I placed two in the bore, and suspended the third freely in the air at the top of the *trepied.* In order to preserve these instruments from the influence of the water, and that I might not have difficulty in raising them afterwards, I had a tin-case made, which was 2½ inches in diameter and 24 feet in length, and which I sank in such a manner that its upper end was a foot below the surface of the ice. I introduced the first thermometer into the bottom of the case, and placed the second at 12 feet higher up; so that the latter was at a depth of 13 feet, and the first at a depth of 25 feet, in the glacier. In order, moreover, to protect my instruments from the possibility of being pressed by the walls of the glacier, I took care to include each of them in a thick sheath of brass. Each instrument is coated with tallow in its sheath, and the tin case which contains both is also equally well closed, so as to prevent the access of water. By this means I hope this year to obtain in these lofty regions the minimum temperature of the glacier, at depths of 25 and 13 feet, and that of the air at 12 feet above the surface. I intend going to ascertain the results whenever the snow has disappeared from the surface of the glacier.

### MODE OF LIVING ON THE GLACIER.

I cannot conclude this article without saying a few words on the kind of life we passed at the *Hôtel des Neuchâtelois;* for I do not suppose that a correct idea can be formed of it without possessing an exact knowledge of glaciers. At first sight there is something extraordinary in the notion of a prolonged sojourn in the midst of a sea of ice, at a height of 7500 feet. It may naturally be imagined that the cold must be excessive, especially at night, and I have frequently been asked how I managed to escape being frozen. The fact is, that it freezes there almost every night, and the thermometrograph often indicated in the air — 4° and 5° (and + 24.8 and 23° F.), and even — 6½° (+ 22°.1 F.). But we were well provided with coverings; and as our abode was sufficiently small, the respiration of five or six individuals sufficed to maintain a tolerable temperature.

Notwithstanding its pompous name, the *Hôtel des Neuchâtelois* is, in reality, but a very small hut about 12 feet long by 6 broad, and 4 high, where its height is greatest. I have already said, that this cabin is situated on the moraine; it has pure ice for its foundation, on which the broad stones of the moraine have been placed so as to form a sort of flooring. A bed of herbs gathered on the sides of the glacier served as a mattress; and to protect ourselves from moisture, we took care to make use of a double covering of wax-cloth. The latter is a precaution which it is important to take, and which I cannot sufficiently recommend to those who wish to live on glaciers; for there, as elsewhere, humidity is much more to be dreaded than cold. As our hut was merely formed of a dry stone wall, we endeavoured to guard against violent winds, by stopping up the interstices with bunches of grass. It nevertheless happened frequently, in spite of our precautions, that a hurricane (*Guxen*) blew fearfully through the wall. As, however, we were generally fatigued by our exertions during the day, we did not sleep the less soundly.

It was the rainy and snowy nights only which were really disagreeable; for as the large block which served as a roof was fissured throughout notwithstanding its enormous thickness, the water penetrated by the fissures, and streamed along its lower surface. Whenever one of these little streamlets encountered an inequality, a cascade was formed which awoke in an annoying manner those who happened to be under it. Sometimes one, and sometimes another, was then seen rising up, and seizing a candle, endeavouring with his finger to give another direction to the troublesome rill. But soon recovering its first direction, it would proceed to moisten the person to the right or left, and thus rouse him by dropping provokingly into his ear or mouth. The unfortunate individual would then get up in his turn, and try to correct the course of the water, or probably send it to sprinkle his companion near him. I remember one night when the rills of water and the cascades were so abundant, that all change of direction was useless; and seeing that it was impossible to shut an eye, we began to amuse ourselves at the expense of our cascades, by communicating to them all sorts of directions. In place of sleeping, we pursued hydrographical studies.

In order to inure ourselves to the cold, several of the party adopted the habit of bathing the body every morning in iced water, in a large tub which the guides placed every evening before the door of the hut, and which in the morning was often covered with ice half an inch in thickness. At first this practice seemed severe, but we soon became accustomed to it, and did not wish to give it up; for after the first disagreeable sensation was surmounted, we were sure to feel warm, and could wear our ordinary dresses with impunity; whereas those who dreaded these icy baths, and did not make use of them, shivered around us enveloped in their cloaks.

Our chief guide, Jacob Leuthold, who was also at the same time our chief cook, arrived between four and five o'clock to prepare breakfast, which generally consisted of a cup of chocolate. When we had finished, the pot was replaced on the fire for the breakfast of our guides, which was cheese-soup. Our first occupation was to visit the thermometrographs and the thermometers; and when the sheath of one or other of these instruments was frozen to the walls of the hole, it became necessary to employ hot water to detach it; an operation which took up a considerable time. Except on rainy days, the boring could not be commenced before eight o'clock, for it was necessary to wait till the rills of water again began to flow the work was then carried on till midday. Those who were not occupied with the borers made an excursion to some neighbouring summit, or visited one of the numerous moraines which descend from the flanks of the valley; and as I had taken with me a landscape painter, M. Bourkhart of Neuchâtel, to delineate the most remarkable phenomena of this *mer de glace*, in a scientific and picturesque point of view, I often accompanied him to point out the places most worthy of attention Most frequently, it was not so much the special object we followed, as the unexpected observations we made, which gave importance to these excursions; and at last we did not fix on any particular plan, but simply walked to some summit, or to some lateral glacier, with the anticipation of reaping an ample harvest of new observations. The numerous facts of detail which were collected in the course of these various excursions cannot come within the compass of a mere article in a journal but I intend soon to publish a summary of them in a supplementary volume to my *Etudes sur les Glaciers;* and I hope that they will contribute more and more to increase the interest which attaches to glaciers, by initiating naturalists into the minute history of a natural phenomenon, which, with all its apparent uniformity, is yet so splendid and so varied. What indeed can be more interesting than this series of metamorphoses, to which frozen ice is subjected, from its fall on the high summits in the form of snow or of small hail (*gresil*), to its transformation into those masses of compact ice which descend like immense solid *coulées* into the midst of our forests, and of our cultivated fields! What can be more worthy of attention than the study of the red snow, that microscopic creation which extends as a rose-coloured tint over immense spaces of the névé, in places where traces of organic life could hardly have been expected to exist! What, moreover, can be more curious than to be able to follow for great distances the junction of the glacier and the rock, to see in some degree the former in the act of polishing and furrowing the walls of its bed, and to study the various effects which all this produces on the rocks, according to their hardness and the nature of their composition! Lastly, let me say, that it was here that we first observed the remarkable phenomenon of the *niveaux des roches polies et moutonnées*, which will enable us henceforth to determine the greatest thickness of the ancient sheet of ice which covered our country at any given point in the Alps, and that with a precision so much the more rigorous, because the traces the ice has left of its action on the rocks on which it has operated, are indelible, and form a striking contrast with the rugged and angular rocks which surmount them.

But let us return to our hut. Mid-day approaches, the whole party are re-assembled round the kitchen fire, and each one brings with him an appetite with which, for sharpness, that experienced on the plains cannot be compared. Although, therefore, the fare was but little varied, all agreed that it was a real enjoyment to dine in the open air at the *Hôtel des Neuchâtelois*, round the large flat block of gneiss which served as our table. We had little else to eat but mutton and rice, but whether it is that the mutton of these high mountains is really better than elsewhere, or that the sharp air renders the palate less fastidious, it is certain that we never tired of it. Sometimes we had for variety some goat's meat, which we likewise found excellent. A cup of coffee and a cigar were the necessary adjuncts to our dinner, and it seemed to us that both the one and the other had a more exquisite perfume under the sky of the Schreckhorn and the Finsteraarhorn. This was the hour for lively conversation, animated discussions, and the proposal of daring projects. After dinner we all returned to our occupations, one in one direction and another in the opposite; or perhaps we remained at the *Hôtel* to write out our notes and observations. The evening thus arrived for the most part more speedily than we could have wished. After the little rills of water on the surface of the glacier began to be dried up, which, in serene days, generally took place between four and five o'clock, the boring was stopped, the holes were emptied of water, and the thermometrographs were introduced, operations which continued till nearly seven o'clock. We then assembled anew round the kitchen; but at that time, although not less hungry than at dinner, we remained a much shorter period at table, for it was much colder, the temperature being then generally about 0 (32° F). The supper being over, we hastened to enter the hut; the light dresses of the day were exchanged for good cloaks and furs; and when night arrived, we closed the curtain which served as a door, and lighted the candle. The guides returned to their habitation on the left bank of the glacier, and all slept quietly on a place of repose which under any other circumstances would have been thought detestable.

In this manner we passed altogether about a month on the glacier of the Aar, without including the excursion to the Jungfrau, which took place on the 28th of August. During all this time we had a crowd of visitors, who were curious to see an establishment of so novel a kind.* Others were attracted by a more elevated motive, the desire to participate in our labours, or to testify by their visit the interest which they took in the investigations we were prosecuting; and, as the happiness which we experience at meeting with persons for whom we entertain a profound veneration, or with whom we are on terms of sincere friendship, was heightened by the beauty of the locality, I felt my heart beat with joy whenever I recognised a friend among the travellers who arrived along with the carrier of provisions about eleven o'clock in the morning. I am proud to be able to name among the number of those who visited me or lived with me at the *Hôtel des Neuchâtelois*, General Pfuel, governor of Neuchâtel, who, notwithstanding his age, performed on foot the arduous journey across the glacier, being unwilling to be outstripped even by the youngest; Lord Enniskillen, whose zeal does not give way before any fatigue, when the progress of science is in question; MM. Adolphe and Alfred de Rougemont, whom we are always sure to meet whenever any object of utility connected with Switzerland is concerned; my excellent friend Professor Studer of Berne; Mr and Mrs Trevelyan; Messrs Guyot, Robertson, Cole, Nicholson, Martins, Canson, &c. Lastly, I have already had occasion to mention among the number of those who took an active part in my labours, M. Escher de la Linth, as well as Messieurs Desor, Vogt, Forbes, and Heath. All shared my habitation at the *Hôtel des Neuchâtelois;* and I took care to have all their names cut on one of the surfaces of the large block which served us as a shelter.

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* Among the tourists who visited us, several came from Grindelwald by the Strahleck; these, it may be well supposed, were much fatigued on arriving at the *Hôtel des Neuchâtelois;* and I had pleasure in receiving them as well as possible. One evening, when, being assembled together round our hut, we were amusing ourselves by observing the effects of light on the flanks of the Finsteraarhorn, we saw a numerous caravan issuing from the angle of the Abschwung. Immediately our glasses were pointed in that direction, to try if we could recognise among the travellers any one of our acquaintance. One of the party was in a *chaise à porteurs.* We all expressed our surprise at this singular mode of travelling; but what was our astonishment to discover that this traveller was a lady. Our irony was at once converted into admiration. Was it possible that a lady had crossed the Strahleck! She was certainly the first who had attempted so difficult a route. It turned out to be an English lady, Mrs C—— of Edinburgh, accompanied by her husband and nine guides. We proceeded to meet them. The lady was very young, and seemed very timid; and Mr C—— informed us that she had nevertheless performed the greater part of the journey on foot, but that her shoes had been torn, and it became necessary for her to be carried, which gave her great annoyance. I invited her to take a little repose, after which the caravan continued its route towards the Hospice of the Grimsel, where it arrived at nine o'clock.

I had intended again to have encamped this year under this same block, with which so many recollections are associated in my mind; but my guides have dissuaded me, by asserting that there would be danger for the colony in doing so, owing to the block being so much fissured. I have, therefore, given orders for the construction of a tent at the side of the old *Hôtel des Neuchâtelois;* and it is there that I hope next June and July to welcome all the friends of glaciers who may choose to pay me a visit.

## *On the Occurrence of Platina and Diamonds in Borneo.*

The occurrence of platina in the East Indian Archipelago is but little known, and it is probably quite new to many that this metal is actually obtained there in large quantity.* Upon this subject we have acquired accurate information from the late Dr Ludwig Horner, the son of the celebrated astronomer and voyager Horner of Zurich. The details deserve to be better known, and are contained in the *Verhandelingen van het Bataviaasch Genootschap van Kunsten en Wetenschappen.* xvii Deel. Batavia, 1839, p. 89. At the south-eastern extremity of Borneo, named Tanah Laut (Seeland), there ends a mountain-chain which accompanies the course of the great river of Banjermassing, and which has been traced to the north of the Equator. The most southern portion of the mountain range is termed the Ratoos Mountains, whose highest summit rises 3168 Parisian feet above the sea, and these are chiefly composed of serpentine, diorite, and gabbro. The valleys and the base of these mountains are covered by a thick deposit of red clay, in which there is an imperfectly defined bed of white quartz pebbles. In the valleys the red clay is from 10 to 20 feet deep, and the bed of quartz pebbles is from 1 to 4 feet thick. It is this deposit which contains the gold in extremely small plates, associated with a large quantity of grains of magnetic iron-ore, and every where with small grains of platina, as well as of iridium and osmium, but not of palladium. The strata repose directly on serpentine, and are evidently derived from it; the red clay having had its origin from the rock itself, and the quartz from the quartz veins which traverse the serpentine in great numbers. This is in the district of *Poelo* (Pulo) *Arij*, where 150 Chinese wash out yearly 750 taël of gold,† whose value amounts to 45,000 Dutch florins.

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* The only other part of Southern Asia where this metal has been met with is Ava.

† A taël is 2 ounces.

The diamond mines lie more to the north, but likewise on the west side of the Ratoos Mountains; there, likewise, a red bed of clay stretches over the surface, 6 to 7 fathoms deep, and there is also a bed, one fathom deep, composed of quartz pebbles, and fragments of syenite and diorite; more rarely a marl occurs, containing recent shells (*Ostræa cardium*). In this deposit the diamonds are distributed, sometimes accompanied by magnetic iron sand, by plates of gold and platina, and by small pieces of native iron. The surest sign of the presence of diamonds is the occurrence of small pieces of black quartz, with disseminated iron-pyrites and platina, which are termed *Batoe* (Batu) *Timahan*, or *Batoe Parak Jatan*.

A considerable quantity of gold and platina is obtained along with the diamonds. In the districts of Goenong (Gunong) Lawak, Tapang, and Oedjong Moerung (Udjong Murong) alone, four thousand workmen are employed in the process of washing.

The occurrence of platina in this district was made known in 1831 by Mr Hartmann, then the resident at Banjermassing. Hitherto no advantage has been derived from his discovery, as the platina is thrown away as entirely useless. It is, however, certain, that a tenth part of platina is washed out with the gold. The produce of Poelo Arij is 1000 taël of gold, and thus 100 taël of platina are obtained; and if we calculate the produce of the washings of the district in the mountains nearer the equator at four times as much, we have 500 taël of platina washed out and cast away. Besides this, there is what is obtained and thrown away on the west coast. According to Crawfurd (History of the Indian Archipelago, iii. p. 482), the whole gold obtained in the Chinese colonies of Mandoor and Montrado, on the west coast of Borneo, is about 88,632 ounces, of which the tenth part, or about 8000 ounces, or 4000 taël, are platina. We are probably under the mark, when we estimate the annual quantity of platina lost in Borneo at 5000 taël, or 10,000 ounces, that is, 625 pounds. (*From Poggendorff's Annalen*, 1842, *No.* 3.)

# *On some Peculiar Changes in the Internal Structure of Iron, independent of, and subsequent to, the several Processes of its Manufacture.* By Charles Hood, Esq., F.R.A.S., &c.*

The important purposes to which iron is applied have always rendered it a subject of peculiar interest; and at no period has its importance been so general and extensive as at the present time, when its application is almost daily extending, and there is scarcely any thing connected with the arts, to which, either directly or indirectly, it does not in some degree contribute. My object in the present paper is to point out some peculiarities in the habitudes of iron, which appear almost wholly to have escaped the attention of scientific men; and which, although in some degree known to practical mechanics, have been generally considered by them as isolated facts, and not regarded as the results of a general and important law. The circumstances, however, well deserve the serious attention of scientific men, on account of the very important consequences to which they lead. The two great distinctions which exist in malleable wrought iron, are known by the names of "red-short" and "cold-short" qualities. The former of these comprises the tough fibrous iron, which generally possesses considerable strength when cold; the latter shews a bright crystallized fracture, and is very brittle when cold, but works ductile while hot. These distinctions are perfectly well known to all those who are conversant with the qualities of iron; but it is not generally known that there are several ways by which the tough red-shot iron becomes rapidly converted into the crystallized, and by this change its strength is diminished to a very great extent.

The importance which attaches to this subject at the present time, will not, I think, be denied. The recent accident on the Paris and Versailles Railway, by which such a lamentable sacrifice of human life has occurred, arose from the breaking of the axle of a locomotive engine, and which axle presented at the fractured parts the appearance of the large crystals, which always indicate cold-short and brittle iron. I believe there is no doubt, however, that this axle, although presenting such decided evidence of being at the time of this accident of the brittle cold-short quality, was at no distant period tough and fibrous in the highest degree; and as the French government have deemed the matter of sufficient importance to be inquired into by a special commission, I trust that some remarks on the subject will be interesting to the members of the Institution of Civil Engineers. I propose, therefore, to shew how these extraordinary and most important changes occur, and shall point out some at least of the modes by which we can demonstrate the truth of this assertion by actual experiment.

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* Read before the Institution of Civil Engineers, June 21, 1842, and communicated by the Author to the twenty-first volume of the Philosophical Magazine, from which periodical we have extracted it.

The principal causes which produce this change are percussion, heat, and magnetism; and it is doubtful whether either of these means *per se* will produce this effect; and there appear strong reasons for supposing that, generally, they are all in some degree concerned in the production of the observed results.

The most common exemplification of the effect of heat in crystallizing fibrous iron, is by breaking a wrought iron furnace bar, which, whatever quality it was of in the first instance, will, in a short time, invariably be converted into crystallized iron; and by heating, and rapidly cooling by quenching with water a few times, any piece of wrought iron, the same effect may be far more speedily produced.

In these cases we have at least two of the above causes in operation—heat and magnetism. In every instance of heating iron to a very high temperature, it undergoes a change in its electric or magnetic condition; for, at very high temperatures, iron entirely loses its magnetic powers, which return, as it gradually cools to a lower temperature. In the case of quenching the heated iron with water, we have a still more decisive assistance from the electric and magnetic forces; for Sir Humphry Davy long since pointed out* that all cases of vaporization produced negative electricity in the bodies in contact with the vapour; a fact which has lately excited a good deal of attention, in consequence of the discovery of large quantities of negative electricity in effluent steam.

These results, however, are practically of but little consequence; but the effects of percussion are at once various, extensive, and of high importance. We shall trace these effects under several different circumstances.

In the manufacture of some descriptions of hammered iron, the bar is first rolled into shape, and then one-half the length of the bar is heated in a furnace, and immediately taken to the tilt-hammer and hammered; and the other end of the bar is then heated and hammered in the same manner. In order to avoid any unevenness in the bar, or any difference in its colour, where the two distinct operations have terminated, the workman frequently gives the bar a few blows with the hammer on that part which he first operated upon. That part of the bar has, however, by this time become comparatively cold; and if this cooling process has proceeded too far when it receives this additional hammering, that part of the bar *immediately* becomes crystallized, and so extremely brittle that it will break to pieces by merely throwing it on the ground, though all the rest of the bar will exhibit the best and toughest quality imaginable.

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* Davy's Chemical Philosophy, p. 138.

This change, therefore, has been produced by percussion (as the primary agent), when the bar is at a lower temperature than a welding heat.

We here see the effects of percussion in a very instructive form. And it must be observed that it is not the excess of hammering which produces the effect, but the absence of a sufficient degree of heat at the time the hammering takes place; and the evil may probably be all produced by four or five blows of the hammer, if the bar happens to be of a small size. In this case we witness the combined effects of percussion, heat, and magnetism. When the bar is hammered at the proper temperature, no such crystallization takes place, because the bar is insensible to magnetism. But as soon as the bar becomes of that lower degree of temperature at which it can be affected by magnetism, the effect of the blows it receives is to produce magnetic induction, and that magnetic induction and consequent polarity of its particles, when assisted by further vibrations from additional percussion, produces a crystallized texture. For it is perfectly well known that in soft iron, magnetism can be almost instantaneously produced by percussion, and it is probable that the higher the temperature of the bar at the time it receives the magnetism, the more likely will it be to allow of that re-arrangement of its molecules which would constitute the crystallization of the iron.

It is not difficult to produce the same effects by repeated blows from a hand-hammer on small bars of iron; but it appears to depend upon something peculiar in the blow, which to produce the effect must occasion a complete vibration among the particles in the neighbourhood of the part which is struck. And it is remarkable that the effects of the blows in all cases seem to be confined within certain limited distances of the spot which receives the strokes. Mr Charles Manby has mentioned to me a circumstance which fully bears out this statement. In the machine used for blowing air at the Beaufort Iron Works, the piston-rod of the blowing cylinder, for a considerable time, had a very disagreeable jar in its motion, the cause of which could not be discovered. At last the piston-rod broke off quite short, and close to the piston; and it was then discovered that the key had not properly fastened the piston and the rod together. The rod at the fracture presented a very crystallized texture; and as it was known to have been made from the very best iron, it excited considerable surprise. The rod was then cut at a short distance from the fracture, and it was found to be tough and fibrous in a very high degree; shewing what I have already pointed out, that the effects of percussion generally extend only a very short distance. In fact, we might naturally expect, that, as the effect of vibration diminishes in proportion to the distance from the stroke which produces it, so the crystallization, if produced by this means, would also diminish in the same proportion. The effect of magnetism alone may also be estimated from this circumstance. The rod would of course be magnetic throughout its whole length; this being a necessary consequence of its position, independent of other circumstances; but the necessary force of vibration among its particles only extended for a short distance, and to that extent only did the crystallization proceed. The effect of magnetism in assisting the crystallization, I think it unnecessary to dwell upon, as the extensive use of galvanic currents in modern times has fully proved their power in crystallizing some of the most refractory substances; but by themselves they are unable to produce these effects on iron, or at least the operation must be extremely slow.

Another circumstance which occurred under Mr Manby's observation, confirms generally the preceding opinions. A small bar of good tough iron was suspended and struck continuously with small hand-hammers, to keep up a constant vibration. The bar, after the experiment had been continued for some considerable time, became so extremely brittle, that it entirely fell to pieces under the light blows of the hand-hammers, presenting throughout its structure a highly crystallized appearance.

The fracture of the axles of road-vehicles of all kinds is another instance of the same kind. I have at different times examined many broken axles of common road-vehicles, and I never met with one which did not present a crystallized fracture; while it is almost certain that this could not have been the original character of the iron, as they have frequently been used for years with much heavier loads, and at last have broken without any apparent cause, with lighter burdens and less strain than they have formerly borne. The effects, however, on the axles of road-vehicles are generally extremely slow, arising, I apprehend, from the fact that, although they receive a great amount of vibration, they possess a very small amount of magnetism, and are not subject to a high temperature. The degree of magnetism they receive must be extremely small, from their position and constant change with regard to the magnetic meridian, the absence of rotation, and their insulation by the wood-spokes of the wheels. Whether the effects are equally slow with iron-wheels used on common roads, may perhaps admit of some question. With railway-axles, however, the case is very different. In every instance of a fractured railway-axle, the iron has presented the same crystallized appearance; but this effect, I think, we shall find is likely to be produced far more rapidly than we might at first expect, as these axles are subject to other influences, which, if the theory here stated be correct, must greatly diminish the time required to produce the change in some other cases. Unlike other axles, those used on railways rotate with the wheels, and consequently must become, during rotation, highly magnetic. Messrs Barlow and Christie were the first to demonstrate the magnetism by rotation produced in iron, which was afterwards extended by Messrs Herschel and Babbage to other metals generally, in verifying some experiments by M. Arago. It cannot, I think, be doubted, that all railway-axles become from this cause highly magnetic during the time they are in motion, though they may not retain the magnetism permanently. But in the axles of locomotive engines, we have yet another cause which may tend to increase the effect. The vaporization of water and the effluence of steam have already been stated to produce large quantities of negative electricity in the bodies in contact with the vapour; and Dr Ure has shewn,* that negative electricity, in all ordinary cases of crystallization, instantly determines the crystalline arrangement. This, of course, must affect a body of iron in a different degree to that of ordinary cases of crystallization; but still we see that the effects of these various causes all tend in one direction, producing a more rapid change in the internal structure of the iron of the axle of a locomotive engine, than occurs in almost any other case.

Dr Wollaston first pointed out that the forms in which native iron is disposed to break, are those of the regular octahedron and tetrahedron, or rhomboid, consisting of these forms combined. The tough and fibrous character of wrought iron is entirely produced by art; and we see in these changes that have been described, an effort at returning to the natural and primal form; the crystalline structure, in fact, being the natural state of a large number of the metals; and Sir Humphrey Davy has shewn that all those which are fusible by ordinary means assume the form of regular crystals by slow cooling.

The general conclusion to which these remarks lead us, appears, I think, to leave no doubt that there is a constant tendency in wrought iron, under certain circumstances, to return to the crystallized state; but that this crystallization is not necessarily dependent upon time for its development, but is determined solely by other circumstances, of which the principal is undoubtedly vibration. Heat, within certain limits, though greatly assisting the rapidity of the change, is certainly not essential to it; but magnetism, induced either by percussion or otherwise, is an essential accompaniment of the phenomena attending the change.

At a recent sitting of the Academy of Sciences at Paris, M. Bosquillon made some remarks relative to the causes of the breaking of the axle on the Versailles railroad; and he appears to consider that this crystallization was the joint effect of time and vibration, or rather, that this change only occurs after a certain period of time. From what has here been said, it will be apparent that a fixed duration of time is not an essential element in the operation; that the change, under certain circumstances, may take place instantaneously; and that an axle may become crystallized in an extremely short period of time, provided that vibrations of sufficient force and magnitude be communicated to it. This circumstance would point out the necessity for preventing as much as possible all jar and percussion on railway-axles. No doubt one of the great faults of both engines and carriages of every description—but particularly the latter—is their possessing far too much rigidity; thus increasing the force of every blow produced by the numerous causes incidental to railway transit; by causing the whole weight of the entire body in motion, to act by its momentum in consequence of the perfect rigidity of the several parts and the manner of their connection with each other, instead of such a degree of elasticity as would render the different parts nearly independent of one another, in the case of sudden jerks or blows; and which rigidity must produce very great mischief both to the road and to the machinery moving upon it. The looseness of the axles in their brasses must also be another cause which would greatly increase this evil.

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* Journal of Science, vol. v. p. 106.

Although I have more particularly alluded to the change in the internal structure of iron with reference to the effects on railway-axles, it need scarcely be observed that the same remarks would apply to a vast number of other cases, where iron, from being more or less exposed to similar causes of action, must be similarly acted upon. The case of railway-axles appears to be of peculiar and pressing importance, well deserving the most serious consideration of scientific men, and particularly deserving the attention of those connected with railways, or otherways engaged in the manufacture of railway-machinery, who have the means of testing the accuracy of the theory here proposed. For if the views I have stated be found to harmonize with the deductions of science, and to coincide with the results of experience, they may have a very important effect upon public safety. It may be observed, on the other hand, however, that at the present time all railway-axles are made infinitely stronger than would be necessary for resisting any force they would have to sustain in producing fracture, provided the iron were of the best quality; and to this circumstance may perhaps be attributed the comparative freedom from serious accidents by broken axles. The necessity for resisting flexure and the effects of torsion, are reasons why railway-axles never can be made of such dimensions only as would resist simple fracture; but it would be very desirable to possess some accurate experiments on the strength of wrought iron in different stages of its crystallization, as there can be no doubt that very great differences exist in this respect, and it is probable that in most cases, when the crystallization has once commenced, the continuance of the same causes which first produced it goes on continually increasing it, and thereby further reduces the cohesive strength of the iron.

Earl Street, *May* 31. 1842.

[Several samples of broken railway-axles accompanied this paper, and were exhibited at the meeting. In some of them the same axle was broken in different places, and shewed that where the greatest amount of percussion had been received, the crystallization of the iron was far more extensive than in those parts where the percussion had been less.]

# *On a Re-arrangement of the Molecules of a Body after Solidification.* By ROBERT WARINGTON, Esq.*

HAVING occasion lately to prepare some alloys of lead for the purpose of lecture-illustration, I was much surprised at an alteration taking place in the arrangement of the particles of one of these alloys, as shewn by the appearance of the surfaces of fracture, after the metal had assumed the solid form. The alloy experimented on was that known as Newton's fusible metal, composed of 8 parts of bismuth, 5 of lead, and 3 of tin. On pouring this alloy, in the melted state, on a marble slab, and breaking it as soon as solid, and when it may be readily handled, the exposed surfaces were found to exhibit a bright, smooth, or conchoidal metallic appearance, of a tin white lustre; and the act of disjunction at one part will, frequently, cause the whole to fly into a number of fragments, analogous to the breaking a piece of unannealed glass.

The metal after this becomes so hot as to burn the fingers if taken up; and when this evolution of heat has ceased, the alloy will be found to have entirely altered its characters, having lost its extreme brittleness, requiring to be bent to and fro several times before it will break, and presenting on fracture a fine granular or crystalline surface of a dark colour and dull earthy aspect. Similar phenomena accompany the casting of the fusible alloy of V. Rose, composed of 2 parts of bismuth, 1 of lead, and 1 of tin.

The fact of the evolution of heat from the alloy of Newton, and its cause, are thus noticed by Berzelius in his *Traité de Chimie*:—"If this alloy is plunged into cold water, and quickly withdrawn and taken in the hand, it becomes sufficiently hot, after a few moments, to burn the fingers. The cause of this phenomenon is, that during the solidification and crystallization of the *internal parts*, the latent heat of these is set free, and communicates itself to the surface before the fixing and cooling." The alteration in the internal arrangement of the particles, as proved by the surfaces of fracture, is not however noticed, and the explanation is defective, as it supposes the interior not to have assumed the solid state until the evolution of the heat occurs. If such were the case, it would be seen on breaking it in the first instance. The phenomena can only be accounted for by admitting a certain degree of mobility among the particles, and that a second molecular arrangement takes place after the metal has solidified. This may arise from their not having assumed, in the first state, that direction in which their cohesion was the strongest.

That a very marked and extraordinary alteration in the characters and properties of various substances arises entirely from this change in the position of their component particles, effected either by the communication or abstraction of heat after solidification, there can be no doubt. And these changes are applied to many very important purposes in the arts and manufactures—such as the hardening and tempering of steel, the rolling of commercial zinc, and rendering that metal permanently malleable, the annealing of glass, and a variety of other uses, particularly in crystallization, which might be adduced.

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* Communicated by the Chemical Society; having been read January 4.—*Phil. Mag.* S. 3, vol. xx, No. 134; Suppl. July 1842.

The following experiments were made to ascertain to what extent the emission of latent heat takes place. The melted alloy was poured, in a perfectly fluid state, on a bulb of a thermometer, placed in a small platinum crucible, having a capacity equal to about 70 grain measures of water, and standing in a vessel of cold water or mercury. The thermometer, surrounded by the solidified metal and crucible, was removed from the cooling medium before it had reached its stationary point, and the greatest decrease of temperature noted. The heat then rose rapidly again, and the maximum effect was registered. The fusing point of the alloy was 202° Fahrenheit, and the following results were obtained:—

| Exper. | | Fahr. | | Fahr. | Diff. Fahr. |
|---|---|---|---|---|---|
| 1 | thermometer fell to | 97° | and then rose to | 157° | 60° |
| 2 | | 94 | | 149 | 55 |
| 3 | | 90 | | 150 | 60 |
| 4 | | 87 | | 147 | 60 |
| 5 | | 104 | | 156 | 52 |
| 6 | | 97 | | 148 | 51 |
| 7 | | 92 | | 152 | 60 |
| 8 | | 104 | | 155 | 51 |

So that in four out of the eight trials, a difference of 60° Fahrenheit was rendered apparent.

In a platinum crucible of larger size, the effects were not so marked, 34° Fahrenheit being the greatest difference obtained; this of course would arise from the greater bulk of the melted metal not exposing comparatively so large a surface to the cooling medium.

## *On a New Method of Illuminating Church Clocks.* By Mr R. Bryson, Edinburgh. (Communicated by the Royal Scottish Society of Arts.*)

The usual methods employed in the illuminating of church clocks at night have hitherto been liable, *first*, to the objection of considerable expense in fitting up the self-acting machinery for lighting and extinguishing the gas, and, *second*, to the indistinctness of the figures at night. To obviate these, I propose the following plan, which appears to offer several advantages.

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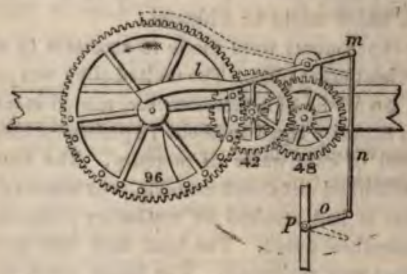
* Read before the Royal Scottish Society of Arts 22d Nov. 1841.

Fig. 1.



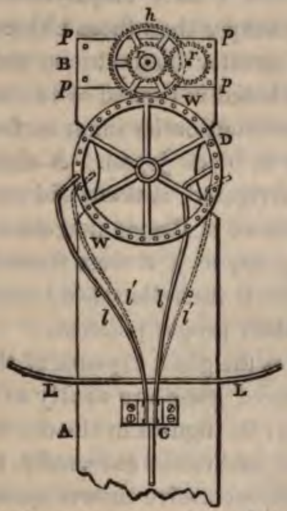
The method now in use is illustrated by fig. 1. The wheels marked 42 and 48 are the usual motion-wheels used in every clock for the purpose of connecting the hands. An additional pinion of twelve is put on the wheel of 42 to turn the wheel 96. This wheel has thirteen pins, one hour's motion apart, which raise the lever *l*, and allow it to fall when the pins have passed. During the time the lever is up (as shewn by the dotted lines), its opposite end *m*, by means of the connecting rod *n*, keeps the lever *o* of the gas-cock *p* down, and thus nearly closes it, allowing the passage of just enough of gas to keep the burners at a blue flame.

When the weight *l* drops, the handle *o* is raised, and opens the stop-cock *p* to its full extent, and the dial is thus illuminated.

Fig. 2 is the method proposed as simpler, and acting with much less friction on the clock. *r r* are the two motion-wheels; *h* is the hour-wheel of 48 teeth, driving the large wheel W W of 96, round the circumference of which are placed a series of holes, screwed to fit two pins with milled heads, one of which is represented at D. *l l* are two levers, both attached to the stop-cock C; L L are portions of the two leading pipes which communicate with the burners. *p p p p* are the 4 pillars on which the plate is supported for he pivots of the motion-wheels *r r* working in. A B is a trong wooden frame to which all the apparatus is attached.

The hour-wheel *h*, which makes two revolutions in the twenty-four hours, having 48 teeth acting into the large wheel W W, it consequently revolves once in the same period in a direction from left to right. When the pin D arrives at the inclined part of the right hand lever *l*, it moves it into the position represented by the dotted line marked *l′* (accented) when its further progress is stopped by a pin fixed to the frame A B.

Fig. 2.



This motion turns the stop-cock C, and lowers the gas at the burners to a small blue flame, which is done at sunrise. The pin D, which has just acted, is placed on the inner side of the wheel W W, as is also the right hand lever *l*. Suppose the other pin to be at the position indicated by the lower W, and moving up to left hand lever *l′* (accented), it will then be moved into the position shewn at the extreme left. As the day lengthens, the pins are gradually removed from each other, to allow them to act at shorter intervals, and brought nearer as darkness increases. Thus, on the longest day, the pins are at their maximum distance, so that the interval between their acting is only three hours; while, on the shortest day, they are at their minimum, and the interval is sixteen hours. The altering of these pins gives very little trouble, as one has only to be changed to the next hole every fortnight.

The friction is so very small, that an ounce weight placed at the extreme end of either lever, is sufficient to shift the stop-cock, and the time which elapses from the first contact with the lever until it has performed its office, is only ten minutes; so that the friction on the clock only extends over twenty minutes during the twenty-four hours, while, in the old method, fig. 1, the lever *l* is exerting a much greater friction during the whole continuance of daylight.

The method which I have employed in constructing the dials, although no cheaper than those hitherto in use, possesses the advantages of greater distinctness, and remaining much longer clean. Each dial is formed of two discs of plate-glass, on the outmost sheet, but on its inner surface, are painted the hours and minutes in black japan. A thin coating of white japan, very finely levigated, is then laid over all, which gives the dial the appearance of Bisque porcelain when illuminated, and appears during day as if it were formed of glazed porcelain. The inner disc is then placed in contact with the outer, and both fixed in their proper position.

This method saves the great expense of the building system, although the sheets of glass are nearly as expensive; but it keeps the dial clean; the figures in the old method being made of cast brass or lead, and raised externally, become receptacles for dust, which each successive shower spreads over the translucent surface, to the evident detriment of its usefulness.

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## *On the Mechanical Arts of Persia.* By James Robertson, Esq., Civil and Mining Engineer, Edinburgh, late in the service of the Shah of Persia. Communicated by the Royal Scottish Society of Arts. With a Plate.*

Although, perhaps, there is little to be gained in a practical point of view from a description of the Persian arts, it may still be interesting to contrast our own highly improved manufactures with those of less advanced countries.

*Carpentry.*—The art of carpentry, as understood in this

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[*] The papers, of which this is an abstract, were read before the Royal Scottish Society of Arts on 14th Dec. 1840, 8th March 1841, and 28th February 1842.

country, can scarcely be said to exist in Persia, the greatest efforts in this department being there confined to the construction of flat roofs of inconsiderable span; and this might be expected, from the circumstance of timber being there exceedingly scarce.

For forming roofs a species of poplar is generally employed, but for other purposes, oak, chesnut, plane, and the other kinds of hardwood are used. The hard timber, as sold in the bazaars, is all of small scantling, as it has to be brought from the forest on the backs of mules or camels.

In accordance with the invariable custom of all Eastern artizans, the carpenter sits upon the ground while at work. Instead of a bench, a strong stake is driven down before him, leaving about 10 inches above the ground, and upon this he rests his work, and keeps it steady with his feet. The facility with which the work is executed, in such a disadvantageous position, has always been a subject of surprise to European workmen. In the royal arsenals, however, English tools are used, and a better system of working has been introduced, under the superintendence of British officers; but in the native workshops, the workmen are still to be seen squatted on the ground; and, when it is considered that they have been accustomed to this position from their infancy, and that their tools are of such a nature as to act with efficiency when used in this way, it is scarcely to be expected that any alteration in their mode of working could be effected by mere example.

The principal tools are the Frame-saw, Plate V., fig. 1. This is somewhat like the English pit-saw, but less in size, and it is used by drawing backwards and forwards; the timber being supported at one end. Hand-saw, fig. 2. The board to be cut up is placed against the stake already noticed, and kept steady with the foot; and as the teeth point backwards towards the handle, the weight of the body assists in giving effect to the instrument. These saws are thin and light, as they have not to resist a thrust like ours.

Adze, fig. 3. This is a most useful tool, and I have noticed English workmen in Persia using it in preference to the axe or paring-chissel for light work.

Planes, figs. 4, 5, 6, 7. As these plane-irons have no cover
the planes are used across the grain of the wood.

Hammer, fig. 8.

Nail, fig. 9. Instead of a head, part of the thick end is be
out thin, and this is turned over with the hammer as the na
is driven down.

Bow and Drill, fig. 10. This is a good instrument, and
used as a brad-awl, gimblet, and brace and bit.

To this list many smaller tools might be added.

*Smithwork.*—As the work on which the Persian blacksmit
have to exert their skill is usually small, their tools are lig
and simple. The iron generally used is of Russian manufa
ture, which is brought from the ports on the Caspian Sea o
the backs of mules. In the northern parts of Persia, ma
leable iron is manufactured directly from the ore; and th
description of iron has been long esteemed for making exce
lent horse-shoes, and horse-shoe nails.

A short time since I described this manufacture in a com
munication to the Royal Society; and as the paper has bee
published in their Transactions, any farther notice of the pr
cess is here unnecessary.

As coal is almost unknown in Persia, the fuel used by th
smiths is entirely charcoal prepared from hard wood. Th
smiths stand when the work requires to be heated, but i
finishing, or making small articles, they sit on the ground.

The hearth is a small platform, without a chimney, havin
a low wall on one side, to prevent the bellows being injure
by the heat.

Bellows, figs. 11, 12, 13. The bellows are double, and th
two nozzles enter the twyre together. They are worked b
a man, who stands between the handles, and by his pushin
forward and drawing back the handles alternately a stead
blast is produced.

Anvil, fig. 14. For small work, a rectangular piece of iro
is often used.

Hammers, fig. 15.

Tongs, fig. 16.

Drill, fig. 17. This instrument is sometimes made of woo
and sometimes of iron, and works remarkably well, notwit

standing its quick motion: it is worked by depressing the small bar, which reascends at every stroke. A similar instrument of smaller size is used in this country in die-sinking.

Horse shoes are made thin and light, with a sharp projecting edge on the outside of the lower rim; this projection, and the large heads of the shoe-nails, enable the horses and mules to keep their feet on smooth rocks or ice. As the greater part of the hoof is protected by the shoe, the foot is not easily injured by sharp rocks; and the small hole in the centre of the shoe enables the animal to lay hold of the sharp projections met with in steep and otherwise impassable tracks, so that the horses shod in this way can climb and descend steep rocky paths with the greatest safety and speed. Sometimes a piece of felt is placed between the shoe and the hoof.

*Turning in wood.*—This operation is performed by a carpenter while sitting on the ground. Two stakes are driven down before him, a short distance apart, and an iron spindle, with a small drum attached, revolves between them. The spindle is passed through the wood which is to be turned, and with the assistance of a bow and string passed round the drum, the spindle is made to revolve rapidly. The bow is worked backwards and forwards by the left hand, while the right holds the cutting-tool supported on a block of wood.

Turning in metal is almost unknown.

*Stone-cutting.*—As the buildings are generally made of clay or brick, stone-cutting is little practised in Persia. Gravestones, millstones, and a few other articles, therefore, embrace the whole of the works of this description. When the work admits of it, the stone-cutters sit upon the ground. The principal tools are, small double-pointed picks, and mason irons resembling large nails, some pointed, and some chisel-shaped. With these tools the stone-cutters work very slowly, and it is only after immense labour that they succeed in bringing a hard stone to the required form.

For boring in stone, the instrument is an iron rod steeled at the end, but instead of a chisel point the end is cut flat off. Two parallel regular grooves are cut deep across this face, and these are intersected by three others at right angles, thus diving the end of the rod into twelve compartments. While boring, the hole is kept full of water, and while the rod i turned round gradually with the left hand, the blows ar struck by a small hammer held in the right. This method o boring is very tedious.

It has just been noticed that one of the principal employments of these stone-cutters is the formation of millstones and perhaps a few remarks on Persian mills may not be ou of place here.

There are two descriptions of corn-mills, the hand-mill an the water-mill. The first is composed of two small circula stones, which are kept together by a peg in the centre of th lower stone, which passes through a large opening in the uppe one; the grain is fed in at this opening, and while the uppe stone is turned round by means of a small peg on its rim, th flour is thrown out at the edges.

For moving the large stone of the water-mill a considerabl fall is required. A hollow tree is placed in a sloping position, from the end of the lead, and the confined water, in rushing from the lower orifice, acts upon the oblique and narrow float-boards of a horizontal wheel, about five or six feet in diameter. A perpendicular iron spindle passes from the water-wheel through the lower stone, and gives motion to the upper one, without any intermediate machinery. These mills are placed on the slopes of hills when water can be commanded; and as they are generally protected by a fortified tower, and surrounded by a luxuriant grove of tall poplars, they form a pleasing feature of the landscape on approaching a Persian village.

*Method of Procuring Water.*—As rain seldom falls in Persia, the farmers have recourse to irrigation for watering their fields. The water used in this process is procured either by means of cuts from the rivers in the vicinity, or more rarely by a system of subterranean canals, which draw off the water from the high grounds. From a want of concert among the inhabitants, the water-courses are seldom carried to any distance; so that immense plains of the richest alluvial soil are met with alongst the banks of most rivers, which, by a little capital and skill, could be made capable of yielding the richest crops, but which at present afford only a transient pasturage f[illegible]rds of the wandering Coords; this is more particularly the case when the rivers afford little fall in their course, as the limited capital of the people precludes any attempt to conduct a canal along an extended district, or the erection of machinery to raise the water even to an inconsiderable height. As far as I know, the celebrated Persian wheel is now unknown in the country from which it derived its name; nor is there any hope of foreigners introducing a better system where property is so insecure. It must be admitted, however, that the inhabitants shew no want of ingenuity or enterprize, when they see clearly the advantages of any undertaking suited to their limited means, and this is strikingly displayed in their mode of procuring water in the populous districts. They sink a series of small shallow shafts in the thick alluvial clay, and connect the pits by means of small mines or levels. A subterranean canal is carried in this way from the flat ground till it meet the slope of the surrounding hills. At this point a cross mine is driven along the face of the hard strata, and numerous small openings are made into the rock, wherever the water makes its appearance. Very considerable streams are often procured in this way, which gush from their hidden sources in a profuse current, after travelling several miles below ground, protected in their course by the clay cover from being evaporated by the overpowering heat of the sun. In travelling through the plains of Persia, immense rows of small hillocks may every where be seen, marking the position of these pits and the lines of the old canals, in districts now quite deserted; and these at once shew the extent to which this system had at one time been carried, and the wretchedness to which the country has now been reduced.

*Brick-Making.*—A level space of ground having been selected, near a stream of water, the grass and vegetable soils are carefully removed. The ground is then broken at one extremity of the prepared platform, and the easily pulverized clay is carefully passed through a small meshed riddle, and placed in the hollow, while the stones and roots are thrown behind. When a sufficient quantity of riddled clay has been collected, a small stream of water is allowed to flow into the hollow, and the mass is brought to a proper consistency by treading. The prepared clay is now deposited in different small heaps upon the floor, which has been previously spread with finely riddled earth. The moulds are formed of thin wood, without any of those projections or handles which are seen in this country. For the common-sized brick, the mould is formed about 9 inches square and 1½ inch deep; but larger bricks are sometimes required, for paving courts and coping walls, for which another mould is necessary.

The mould is placed on the ground, and the brickmaker takes a part of the clay in his hands, and places it loosely in the mould. He then dips his hands in water and throws a little of it around the inside of the mould, to prevent the clay from adhering to the wood. By a peculiar action of the hands the clay is then drawn from the middle and pressed firmly into the corners and round the sides of the mould, and the whole is afterwards levelled over, by a dexterous diagonal stroke of the right hand. The mould is now lifted off the brick, and placed to the right-hand side, close to, and in the same line with, the brick already formed, and it is again filled up in the same way. Thus he proceeds, frequently washing the mould in water, till a straight line of bricks has been laid down, of many yards in length; a second line is then commenced, exactly the thickness of the mould from the first, and the whole ground is finally covered with closely arranged rows of bricks.

In two days or more, when the level space has been covered, the first made bricks become sufficiently dried to be handled, and the brickmaker now proceeds to place them upon edge, in lines; in a day they are sufficiently hard to be removed, and are then carried to a convenient spot, where they are built up edgeways in the form of a wall, one brick in thickness, with small openings between them, for the circulation of air. Whenever 20,000 or 30,000 have been collected, they are removed to the kiln, to be burned; or if sun-dried bricks only be required, they are now ready for use.

As coal is almost unknown in Persia, the brickmaker has recourse to a kiln of singular construction, well suited to the fuel he has most at command. To those unacquainted with Eastern economy, it may appear surprising that the fuel used by the lower classes, and even by persons in affluent circumstances, is formed of the refuse of the stable and cow-house.

As this substance, however, emits little flame, the brick-kiln has to be supplied with withered plants and bushes, which are collected in abundance on the hills, whose strong though transient flames ascend through the interstices of the closely packed bricks.

The brick-kiln may be shortly described as a small vault, dug out of the ground, and surrounded by a wall of sun-dried brick, having a door-way at each end for receiving the fuel. It is closely covered by a series of very narrow arches, the top of which forms the floor on which the bricks are placed; and this again is surrounded by a brick wall, with door-ways for putting in and removing the bricks. See fig. 18.

The bricks are arranged on edge in the kiln, and the doorways are built up. A regular supply of fuel is kept up, by the two lower doors, until the required quantity has been consumed. During the first two or three hours, clouds of white vapour ascend from the top of the kiln. When this appearance has ceased, and thick volumes of dark smoke begin to arise, two or three layers of unburnt bricks are laid flatways over the top of the kiln. In about twelve hours the whole of the fuel has been thrown into the vault, and the two feeding door-ways are built up. The kiln is allowed to remain in this state for two or three days, and, when perfectly cool, the burnt bricks are removed for use.

The bricks, when well prepared, are of a fine red colour, and of considerable hardness; but, from the mode of manufacture, one side is always exceedingly rough and uneven, but this is no disadvantage, as the joints in buildings are seldom less than one inch and a half thick.

It may become a question whether the method above described might not be successfully imitated in our own country, particularly in those inland and secluded districts where coal is dear, and yet where a demand exists for tiles and bricks for agricultural and other purposes.

It is difficult to compare the Persian process with our own, but it may be noticed, that, in this country, a workman can mould about 700 paving tiles in a day, of exactly the same dimensions as the Persian bricks, but then the clay is prepared by machinery, and the moist tiles are removed by other hands. The price of these tiles, when made of common clay, is L.5

per 1000. A Persian will prepare the clay and make about 2000 per day, and the selling price is only 9s. per 1000; but it need scarcely be remarked that the bricks are of very inferior workmanship.

*Lime-burning.*—The kiln is constructed in the following manner. A circular excavation, of about ten feet diameter, and six feet deep, is made in the ground in a dry situation, and a sloping pathway cut down to the bottom on one side. A circular wall of rubble stone, two feet in thickness, is then built round the interior of the excavation. At five feet from the foundation, a scarcement of six inches is left in the inside, and the wall, thus diminished, is carried up six feet higher. A small door-way is left opposite the path, and another small opening immediately above it. A narrow door-way is also left in the upper part of the wall, but on the opposite side. See fig. 19.

In charging a kiln of this description a layer of limestone is first laid all round the scarcement, each piece projecting a little inwards, and these are firmly packed and wedged together. A second layer, of larger pieces, is then laid over the first, which also projects inward beyond the first course; a series of similar courses succeed, each being of less diameter than the preceding, the whole forming a dome-shaped ceiling of limestone, which is closed by a single stone at the top. Great attention is required, during the filling up, that the larger masses of limestone may be laid lowest and nearest the centre, and the smaller pieces towards the wall and upper part of the kiln. The door-way, in the upper part of the wall, is now built up, and a conical hill of chips thrown on the top of the broken limestone, in the kiln, and it is now ready for being lighted. A sufficient quantity of furze or withered bushes having been collected, near the kiln, the workman throws gradually, through the feeding aperture, so many loads of fuel as he judges necessary, to calcine the limestone. When the white vapour and smoke disappear, then a quantity of lime-riddling, ashes, or other refuse, is spread over the top of the kiln, and the feeding door-way built up. The kiln remains in this state for two or three days, and is then emptied by opening the upper door-way, and removing the burned limestone, including the stones used in forming the dome. The ashes are used in the manufacture of soap.

With a few improvements, this kiln might be used economically for burning lime, where peat is abundant, and even in most large farms in this country, as much wreck and clippings are burned on the ground every year, as would be sufficient for calcining several kilns of limestone in this simple manner.

*Building.*—Almost all the buildings in Persia are constructed either of clay or bricks.

*Clay-Building.*—The clay is generally procured near the intended erection, and is brought to the proper consistency by mixing with water and treading with the foot. For walls, a foundation is cut out as far down as the vegetable mould, and this trench is filled up with small stones and clay. The walls are built in courses, of about one yard in thickness, each course being allowed sufficient time to consolidate before another is laid. The workman stands upon the top of the wall, and being supplied with pieces of clay by an assistant below, he elevates his arms and throws the mass forcibly down, and then treads the pieces more firmly together with his feet.

The layers are brought to the required batter, and smoothed on the outside, by means of a flat cutting spade. The heat and extreme dryness of the climate, soon render a wall of this description hard and firm, and they last a very long time, as rain seldom falls. Most Persian villages are surrounded by high walls of this kind, having flanking towers at every angle, and a rude ditch in front, from which the materials were excavated, and even the fortifications of the principal cities are constructed of the same material. Almost all the houses are also built in this way, and it is only when room is valuable that thin partition-walls are erected of bricks.

*Brick-Building.*—Of this there are two kinds,—building with sun-dried brick, and with kiln-dried brick ; the method of building, however, is much the same for both. The mortar is generally clay, mixed with chopped straw, and sometimes containing a small proportion of lime. While building, the workmen do not use a trowel, but lay the mortar with the hand. The bond is simple, as the bricks are square, and do not admit of much variety of arrangement. The mortar-joints are usually from one to two inches thick and very irregular, unless in arches or door-ways, when a good deal of neatness is often exhibited. As timber is very scarce, brick-arches and domes are common. The semi-cylindric arched roof is built in this way; after the side walls and gables of the space intended to be covered have been erected, the curve of the arch is marked out upon one of the gables, and this is plastered over with the common clay-mortar; a layer of brick is then stuck upon the mortar; and as the bricks are thin and light, they remain firm till the ring is completed, and then small chips are pinned into the joints, at the opening ends. When one layer is finished, it is plastered over with mortar, and a second layer is stuck upon it in the same manner. In this way an arch of any required length, and of considerable span, is quickly constructed, *without centering.* If the bricks were made sufficiently thin and light, this mode of building would answer well in this country for arching tunnels and drains, and for mining purposes.

Large spaces are often covered over by a brick-dome, or by a series of domes supported on pillars. The pillars being built, thin arches are thrown with the assistance of a slight centering from pillar to pillar, thus dividing the space to be covered, into square compartments. The domes are then completed in the way described for the common arch, without centering, the workmen placing layers of brick on the four sides, alternately. These layers get shorter and shorter as the work proceeds towards the centre, and the workmen, judging by his eye alone, gives the whole intrados such a curve as to form a neat dome when completed. When the domes are very large, stucco is used as mortar, and the bricks, instead of being placed on edge, have their faces downwards, and their edges joined together by the cement.

For light walls, hollow building is common. The first course is of one brick on bed; in the second, two rows of bricks are placed on edge, forming the two faces of the wall, and an upright brick is placed across at every joint; the third course is brick on bed again, and so on. This kind of building would answer well in this country for the upper parts of garden-walls, and generally for building when strength is not required.

The roofs of dwelling-houses are commonly flat, and formed of poplar-trees, neatly peeled—small laths are placed across the beams, and a coarse mat, made of reeds, is placed on the top; a layer of furze is laid over the mat, and the whole is covered by a considerable thickness of clay; the top of the clay is gently sloped, and rendered impervious to water, by being coated repeatedly with clay and chopped straw.

In the houses of the wealthy, the roof is lathed and plastered in the inside, and often beautifully painted and gilt.

The walls of inferior houses are plastered with clay and chopped straw, which has a neat clean appearance; while the apartments of the rich are beautifully finished with stucco, which is either left plain, or decorated with gilding and painting.

It is unnecessary to narrate the arrangement of Persian houses, as good descriptions of these are to be met with in the works of recent travellers: and the writer has throughout limited his observations to such processes as have not been hitherto noticed.

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## *On Nebulæ.* By M. Arago.*

*Definitions.*—The above is the name applied to the *diffused spots* which astronomers have discovered in all parts of the heavens. These spots or lights appear to depend on two entirely different causes, of which it is necessary to give some brief explanation.

The stars are very unequally scattered through the firmament. In certain regions they are crowded; elsewhere we may traverse very extensive spaces, either with the eye or with a glass, without perceiving one. This general want of uniformity in the richness of the starry heavens, has not been properly studied till our own times. It has led to some magnificent results respecting the constitution of the universe, of which we shall soon make mention. At present we have to do only with certain agglomerations of stars, which are local and very circumscribed; such, for example, as the group of the Pleiades, the mass with which *θ* Argûs is surrounded, or that which has been remarked in the constellation Cancer, and which bears the name of *Præsepe*, &c. &c.

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* From the Historical and Critical Analysis of the Life and Works of Sir William Herschel, in the *Annuaire* pour l'an 1842, presenté au Roi par le *Bureau des Longitudes.*

To every short-sighted person, the Pleiades have the appearance of a *confused mass of light;* but when a glass is used, which does not magnify, or the vision is rendered distinct by simple spectacles, the principal stars of this group are seen separately, and become detached, I may say, from one another. The Pleiades, then, are a nebula only to certain observers, and even that only when they do not use spectacles. In the group of Cancer, the different stars being more condensed, the *natural* human vision cannot separate them; the light of one star becomes extended and scattered on the retina, mingles with the light of the neighbouring star, on account of the imperfection of our organs, and the whole forms a confused mass. Avail yourself, on the contrary, of a telescope, even of small power, and the image of each star becomes greatly concentrated, is thus separated from the image of the contiguous star, and the luminous mass loses the character of diffusion, which can only be legitimately maintained in the class of true nebulæ.

In order to attain this result, simple spectacles and a weak glass have been found sufficient when we observe the Pleiades and the group of Cancer. There are luminous spots which we cannot *resolve* into groups of stars, but by the aid of the best telescopes and strong magnifying powers. Those which have resisted magnifiers of 50, 100, 150, and 200 times, give way under magnifiers of 500, 1000, and upwards. It was thus that Herschel succeeded in transforming into agglomerations of stars, the greater part of the nebulæ which Messier, who used less powerful glasses, believed to be irreducible, and which he called nebulæ without stars.

*Nature of Nebulæ.*—The considerable number of nebulæ which, when viewed with ordinary instruments, seem luminous clouds, and of which Herschel effected the decomposition into stars by means of telescopes of 10, 20, and 40 feet, led this great astronomer to a rash generalization. He maintained for many years, that all nebulæ are masses of stars; that there is no other essential difference between nebulæ, the most dissimilar in appearance, but a greater or less distance, or a greater or less condensation, in the stars composing them. He thus placed himself in direct opposition to Lacaille, who, on his return from the Cape of Good Hope, wrote thus, in the Memoirs of the Academy of Sciences for 1755:—"It is not certain that the whiteness of these parts (the clouds of Magellan and the whitenesses of the Milky Way) is caused, as is commonly supposed, by masses of small stars more thickly crowded together than in other parts of the heavens; for with whatsoever attention I examined the best defined extremities, whether of the Milky Way, or the Magellan clouds, I perceived nothing with a glass of 14 feet, but a whiteness in the depth of the sky, without observing more stars than in other places where the sky was obscure." Minute and very delicate observations, made in entire good faith, at last induced Herschel to modify his first opinions. In a memoir of 1791, we find the following words:—"There are nebulosities (whitenesses) which are not of a starry nature." Once having come to the opinion that there exist in the celestial spaces numerous masses of diffused and luminous matter, Herschel saw a field of research open before him, almost entirely new, and which he explored, in all its parts, with indefatigable zeal. The amount of nebulæ then surpassed the restricted limits which had been usually assigned to them; his object was no longer merely to remove uncertainties and the mistakes of astronomical observers; to prevent the wandering comet, even from the time of its first appearance, from being ever confounded with an immoveable nebula, notwithstanding the apparent resemblance in their physical constitution and their great similarity of form. It came to be well understood, from that period, that stars, planets, satellites, and comets, were not the only objects to which the investigations of astronomers ought to be directed. The non-condensed celestial matter,—the celestial matter nearest, if the expression may be allowed, to the elementary state, appeared not less worthy of attention, and presented itself to minds embued with some philosophy, as a fruitful source of discoveries.

*Historical Sketch of the Discovery of Nebulæ.*—The first nebula of which mention is made in the annals of astronomy, is the nebula of *Andromeda.* It was observed by Simon Marius in 1612. This astronomer compared the light of the nebula of Andromeda to that of a candle seen through a thin plate of horn, and the comparison is not inaccurate. Nearly half a century had elapsed from the time of Marius, when, in the year 1656, Huygens observed the large nebula in the constellation Orion. In 1716, Halley, when enumerating the known nebulæ, found that they amounted only to six: the two already mentioned; one, the discovery of which he ascribes to Abraham Ihle, but which, before 1665, had already been noticed by Hevelius; it is between the head and the bow of Sagittarius; the nebula situated in the Centaur, which Halley discovered in the year 1677, while he was working at the catalogue of stars in the southern heavens; the nebula near the right or northern foot of Antinoüs, which Kirch observed in 1681; finally, a nebula, the discovery of which is also due to Halley, situate in the constellation Hercules, on a straight line drawn from $\zeta$ to $\eta$ of Bayer.

During his residence at the Cape of Good Hope, Lacaille fixed the position of 14 nebulæ, in which his feeble instruments shewed nothing definite; and that of 14 others, which the same glasses, on the contrary, decomposed into stars. A few years afterwards, the amount of these objects was notably extended. Messier's catalogue, communicated to the Academy in 1771, and inserted, with some additions, in the *Connaissance des Temps* of 1783, contained 68 nebulæ, which, with the addition of Lacaille's 28, formed a total of 96. This branch of science made the most rapid progress, as soon as Herschel brought to its service his powerful instruments, a rare degree of penetration, and indomitable perseverance. In 1786, this learned philosopher published, in the 76th volume of the *Philosophical Transactions*, a catalogue of a THOUSAND nebulæ, or masses of stars. Three years afterwards, to the great astonishment of observers, a second catalogue appeared, as extensive as the first. To that succeeded, in 1802, a third catalogue of five hundred new nebulæ. *Two thousand five hundred* nebulæ—such, then, was Herschel's contribution to a branch of astronomy scarcely entered upon before his time. The extent of it, at the same time, is the least merit of this great work, as we shall see.

### RESOLVABLE NEBULÆ.

*Their form.*—Nebulæ—even those to which that name is improperly given, or which can be resolved into stars by means of powerful telescopes,—present themselves under a great variety of forms. There are some of them which, being greatly elongated and very narrow, may almost be taken for simple luminous lines, straight or serpentine; others, opening in the shape of a fan, resemble an aigrette diverging from a strongly electrified point. In some cases, the contours have no regularity; in others, one would suppose they had the head and nucleus of a comet. Let us attend to more detailed definitions.

*Circular Nebulæ.*—The circular form is that which resolvable nebulæ appear most commonly to assume. Herschel devoted himself to the examination of circular nebulæ in a most particular manner. He has deduced from his observations important results, of which I shall endeavour to give an exact idea.

The circular form is only apparent; the real form must be globular or spherical. An observation which I shall immediately refer to will render this evident.

In general, the stars of which these nebulæ are composed appear to be very nearly of the same size.* They are distributed around the centre of the figure with perfect regularity. Accordingly, at equal distances from this centre, the luminosity is absolutely equal in all directions.

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* Although the rule I have prescribed for myself prevents me entering upon memoirs posterior to those of William Herschel, I cannot resist the temptation of bringing forward in this place two curious observations by James Dunlop. This astronomer, during his residence at *Paramatta,* New Holland, remarked, at $11^h\ 29^m\ 20^s$ of right ascension, and 29°16′ of southern polar distance, a resolvable nebula of 10′ diameter, in which shone three *red* stars and a *yellow* one, displaying these peculiar kinds of light in the midst of a multitude of white stars. On another occasion,

If we place at a very great distance a spherical nebula, in which the stars are equally condensed in the centre, edges, and throughout; the eye will misrepresent this composition. Let us bring the visual ray which traverses the sphere near the margin. The space comprised between the point of entering and issuing will be very short; the ray will therefore fall upon very few stars. In proportion as this visual ray approaches the centre, the part comprised in the sphere will become longer, and the number of stars it encounters will go on increasing. The maximum will be observed in the centre itself.

The gradual augmentation of intensity from the margin to the centre presented by all nebulæ apparently circular, may thus be considered as a manifest proof of the globular form, of the spherical shape of the starry group.

It is easy to push these considerations further.

We have stated that the parts of the visual rays which are comprised in a sphere, go on increasing in size from the margin to the centre. If the sphere is filled with stars equally distant, the lengths of these parts of the visual rays will be proportioned to the number of stars which the rays touch upon; they will give *the measure* of the luminous intensity of all the regions of the nebula from the edge to the centre. Well, let us bring nearly parallel lines across a sphere. Near the edge, these lines *will vary in length* rapidly; near the centre, on the contrary, they *will vary* very little. The nebula ought, therefore, *to vary* in splendour very rapidly at the edges, and scarcely at all in the centre. This is the reverse of what is witnessed. There must be something inaccurate, therefore, in the hypothesis with which we set out; we must have been wrong in supposing that stars exist in all the parts of the

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his powerful telescope, directed to 18$^h$ 49$^m$ 5$^s$ of right ascension and 53° 10′ of polar distance, presented a nebula to his view of 3½′ diameter, *composed entirely of bluish stars.*

sphere in a state of equal concentration. The rapid augmentation of intensity towards the centre, the presence of a kind of luminous nucleus in the centre itself, prove that the stars are more condensed there, and around it, than in any other place. Such a result is important, at once by its nature and its generality. It ought to be considered as an obvious indication of the existence of a clustering power directed from all parts towards the centre of the globular group.

*Number of stars contained in certain globular Nebulæ.*—It would be impossible to give a detailed and exact enumeration of the total number of stars of which certain globular nebulæ are composed; but we may arrive at certain limits. By taking account of the angular spacing of the stars situate near the edges—that is to say, in the region where they do not project one over another, and comparing it with the total diameter of the group, we ascertain that a nebula, whose diameter is about 10 minutes, and whose apparent superficial extent is scarcely equal to a tenth of that of the lunar disc, contains no less than *twenty thousand stars.*

The dynamical conditions fitted to secure the indefinite preservation of such a multitude of stars, cannot easily be imagined. Are we to suppose that the system is in repose? The stars would in time fall upon each other. Are we to assign to them a rotatory movement round a single axis? Shocks would become inevitable. Further, is it proved, *a priori*, that the globular systems of stars must be preserved indefinitely in the state in which we now see them?

*Perforated or Annular Nebulæ.*—Herschel classed among the curiosities of the firmament a nebula previously inserted under No. 57 in the old catalogue of the *Connaissance des Temps.* But that justice may be done, let us hasten to add that Messier and Mechain, with their feeble glasses, had neither perceived any star in the nebulosity, nor discerned its real form.

This nebula appears as a somewhat elliptical ring of stars. A dark hole is seen in the centre. The two axes are in the proportion of 83 to 100. The obscure opening occupies about the half of the diameter of the nebula.

*Nebulæ are not uniformly disseminated through all the regions of the heavens.*—Herschel, on first entering on the study of nebulæ, made an important remark; he found that they generally form strata. One of these strata is very broad, and lying in a direction almost perpendicular to the Milky Way; it is the stratum in which the Great Bear, Cassiopea, Berenice's Hair, and Virgo, are found. In the middle of one of the strata in question, Herschel perceived no fewer than 31 perfectly distinct nebulæ in the short interval of 36 minutes.

*Nebulæ considered in their relations to the surrounding spaces.*—The spaces which precede and follow simple nebulæ, and still more grouped nebulæ, generally contain very few stars. Herschel found this rule invariable. Accordingly, whenever it happened, after the lapse of a short time, that no star was brought by the motion of the heavens within the range of his fixed telescope, he was accustomed to say to the secretary who assisted him, " Make ready to write, nebulæ are just approaching."

*The spaces poorest in stars are near the richest Nebulæ.*—In the body of Scorpio there is a space of *four degrees* in breadth in which no stars are to be seen. On the western edge of this vast obscure opening, lies the nebula marked 80 in the catalogue of *Connaissance des Temps*, which Herschel considered the richest and most condensed mass of stars which the firmament can offer to the contemplation of astronomers.

The same phenomenon recurs near the fourth nebulous group of the *Connaissance des Temps.* This group is likewise situated on the western edge of a space which contains no stars.

Let us connect these facts with the observation which has shewn that the stars are greatly condensed towards the centre of spherical nebulæ, and with that which has afforded the proof that these stars sensibly obey a certain power of condensation (or clustering power), and we shall feel disposed to admit with Herschel, that nebulæ are sometimes formed by the incessant operation of a great number of ages, at the expense of the scattered stars which originally occupied the surroundng regions; and the existence of empty, or *ravaged* spaces, to e the picturesque expression of the great astronomer, will no longer present anything which ought to confound our imagination.

### NEBULOUS MATTER.

Let us pass from nebulæ resolvable into stars by the aid of powerful telescopes, to those which have never been subjected to such decomposition, and turn our attention to those masses of diffused luminous matter scattered here and there in the firmament.

*The diffused matter occupies very extensive spaces in the heavens.*—Herschel published, in 1811, a catalogue of 52 diffused nebulæ, not resolvable, or at least not resolved, into stars, among which some are to be found extending to 4° 9′ in one of their dimensions. The apparent superficial extent of one of them exceeds that of nine circles of a degree in diameter. The superficial extent of the whole together amounts to 152 of these circles, which is about the 270th part of the number of such circles which form the entire surface of the firmament.

*The great luminous spots have no regular form.*—The forms of the very large diffused nebulæ do not appear susceptible of definition; they possess no regularity. They are found with their contours rectilinear, curvilinear, and mixtilinear. Certain spots terminate distinctly, abruptly, and strongly marked on one side, while on the opposite side they mingle with the light of the sky by an insensible degradation. There are some which throw out very long arms to a distance; in others, large obscure spaces are to be observed in the interior. All the fantastical figures assumed by clouds carried along and agitated by violent and often contrary winds, are found repeated in the firmament of diffused nebulæ.

The diffused nebulæ of a rounded form are not of great dimensions compared with the others. Sometimes (and this circumstance appears highly deserving of attention) there exists between these rounded, very distinct, and well circumscribed nebulæ, a very slender thread of nebulosity attaching them together by their circumferences; one might call it a kind of index, a visible witness to their common origin.

*Of the Light of true Nebulæ.*—Starry nebulæ have been regarded for a long time as true nebulæ. We must not, therefore, expect to discover dissimilarities of a very decided character between the lights of these two natures of bodies. Nebulæ, composed of a diffused, continuous, phosphorescent matter, have, however, quite a peculiar and indefinable aspect, with which the most ancient observers who had an opportunity of examining the heavens with good glasses, appear to have been particularly struck.

Halley, for example, did not hesitate to regard the light of the nebulæ of Orion and Andromeda, as depending on quite a particular cause. "In reality," he says, "these spots are nothing else but the light coming from an extraordinary great space in the ether, through which a lucid medium is diffused that shines with its own proper lustre."*

Derham is not less explicit; the light of nebulæ, according to him, could not be that of a congregation of stars. He even proceeds to ask if, as many philosophers formerly believed, there may not exist beyond the sphere of the remotest stars, a region entirely luminous, an *empyrean* heaven, and if these nebulæ be not this shining region seen through an opening, a chasm, of the sphere (probably crystalline) of the *primum mobile*.

Voltaire mentions Derham's opinion in one of his ingenious romances.

"Micromegas," he says, "traversed the Milky Way in a short time; and I am obliged to confess that he never saw, across the stars with which it is sprinkled, the beautiful empyrean heaven which the illustrious vicar Derham boasts of having seen at the end of his glass. Not that I allege that M. Derham saw wrong; God forbid! but Micromegas was upon the spot; he is a good observer, and I will contradict no one."

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* We find in the Memoir from which I extract this passage, a remark, which is the more singular, as it was made by a man who almost openly professed infidelity. "This," wrote the friend of Newton, "seems fully to reconcile that difficulty which some have moved against the description Moses gives of the creation, alleging that light could not be created without the sun. But in the instances of Nebulæ the contrary is manifest; for some of these bright spots discover no sign of a star in the middle of them."

It would be impossible to criticise Derham's strange conception in a better spirit. I am only astonished that Voltaire, who knew every thing, did not remember that the author of the *Astro-Theology* was not the inventor of the *empyrean.* Anaxagoras alleged that the upper regions (the ether) were filled with fire. Seneca had said: *Openings* are sometimes formed in the heavens, through which we perceive the *flame* which occupies the interior. In describing the nebulæ of Orion, Huygens expresses himself thus: "One might say that the celestial vault, having opened in this place, permits us to behold the more luminous regions beyond."

Finally, if such authorities, from their antiquity, do not appear to establish with sufficient evidence the fact that there is something characteristic in the light which emanates from true nebulæ, I shall cite the recent words of Herschel the younger; "In all the (resolvable) nebulæ, the observer remarks (whatever may be the magnifying power) shootings forth as from stars, or at least he believes that he feels as if he would perceive them if his vision became more distinct. The nebula of Orion produces an entirely different sensation, giving rise to no idea of stars."

*Distribution of the phosphorescent matter in true Nebulæ—Modification which attraction produces in it with the lapse of time.*—The light of these great milky spots is generally very feeble and uniform; here and there only, we remark some spaces *a little more brilliant* than the rest.

On what can this augmentation of intensity depend? Does it depend on a greater *concentration*, or a greater *depth* in the nebulous matter? The choice between these two explanations is not a matter of indifference.

The places where a comparatively bright light is observed in these great nebulosities, are commonly *of small extent.* If, then, we wish to ascribe the phenomenon to the greater depth of the nebulous matter, it is necessary to suppose that a kind of column of the same matter corresponds to each of the points in question; a rectilinear column, very condensed, and *directed exactly towards the earth.* This specialty of direction may seem possible in such or such particular point. It could not be the same either for the whole of the circumscribed radiating places presented by the whole firmament, nor even for two, three, or four of these places which are remarked in a single nebula. It must, therefore, be admitted, that it is the produce of a condensation, an increase of density in certain points of the nebulous spaces, the vast extent of which we have already computed.

Is this condensation the effect of an attractive force, analogous to that which predominates over and regulates all the motions of our solar system? Such is the magnificent problem which we must now endeavour to solve.

In after times, it will be sufficient to throw a double glance, one on the nebulæ of the period, and another on the drawings, so admirable for their delicacy and fidelity, which astronomers of the present day have given of them, to enable the question to be decided, whether time sensibly alters the dimensions and forms of these mysterious groups; but antiquity having left no term of comparison in this respect, we are reduced to the necessity of encountering the problem by indirect means. However, I have every reason to hope that the solution of it will not appear much the less evident.

The phenomena, which the existence of diverse centres of attraction, spread over the whole extent of a single and vast nebula, ought to produce, would develope themselves in this order:—

Here and there, the disappearance of the phosphorescent light; the commencement of breaks in the continuity, or *rents* in the primitive luminous curtain, the necessary result of the motion of the matter towards the attractive centres;

The increase of the rents, that is to say, the transformation of a single nebula into many distinct nebulæ, but little distant from each other, and sometimes connected by very delicate fillets of nebulosity;

The *rounding* of the exterior contour of the separate nebulæ; an augmentation more or less rapid of their intensity from the circumference to the centre;

The formation at this centre of a nucleus, very apparent either by its dimensions or its splendour;

The passage of each nucleus to a stellar state, with the continuance of a slight surrounding nebulosity;

Finally, the precipitation of this last mentioned nebulosity, and, as the definite result, as many STARS as there were distinct centres of attraction in the original nebulosity.

And in what length of time can a single and the same nebulosity undergo all this series of transformations? Of this we are absolutely ignorant. In some instances, perhaps millions of years would be necessary; in other instances, with other conditions of extent, density, physical constitution, and phosphorescent matter, much shorter periods would be sufficient, as the sudden appearance of the new star of 1572 seems to indicate.

The unequal rapidity of the transformations leads to one important consequence. In departing from this basis, it is evident that the nebulæ, if they were all of the same age, must, *taken altogether*, present the various forms which I have enumerated. To one region, ages would scarcely bring a visible accumulation of phosphorescent matter round some centres of attraction; towards another region, owing to a more precipitate movement of concentration, we should already find groups of nebulæ with a nucleus; nebulous stars would at last present themselves here and there, as the last step leading to stars properly so called.

All these states of the nebulous matter indicated by theory, observation had discovered beforehand. The argument is as satisfactory as could be desired; only, instead of following the transformations in a single nebula step by step, their development and progress have been determined by observations made on them collectively. Is it not thus that the naturalist acts, when he is compelled to describe, for all ages, the habit, size, form, and external appearances of the trees composing the forests he is rapidly crossing? The modifications which a very young tree shall undergo, he perceives distinctly and unequivocally with a glance of the eye at an object of the same kind which has already arrived at the most complete degree of growth and development.

*Historical details on the transformation of Nebulæ into Stars*—

*Examination of the difficulties which these ideas of transformation have raised.*—It has been enough for us to group conveniently the diverse forms which diffused nebulæ affect, in order to arrive at the most important cosmogonical conclusion. By means of the natural and sober combination of observation and reasoning, we have established, with a high degree of probability, that a gradual condensation of the phosphorescent matter leads as the last term to sideral appearances; *that we at last arrive at the formation of true stars.*

This bold idea is not so new as is imagined. I can, for example, trace it back as far as Tycho-Brahé.*

This astronomer, in fact, regarded the new star of 1572 as the result of the recent agglomeration of a portion of the diffused matter, disseminated throughout the whole universe, which he called celestial matter.

According to him, celestial matter existed in the *Milky Way* in much greater abundance than elsewhere. Must we then be surprised, he says, that the star should have made its appearance in the midst of this luminous band? Tycho even saw an *obscure space*, as large as the half of the moon's disc, in the very place where the star appeared. He had no remembrance of having observed it before.

Kepler, in his turn, composed the new star of 1604, of the agglomerated matter of ether. This matter, when in a less complete state of condensation, seemed to him the physical cause of the atmosphere with which the sun is enveloped, and which shews itself under the appearance of a feeble luminous crown during the whole continuance of total eclipses of the sun. The new star of 1572 was formed in the Milky Way; the new star of 1604 was not far distant from it. Kepler saw in this coincidence a plausible reason for assigning to the two stars the same origin; only he added: if the milky matter continually engenders stars, why is it not exhausted? Why does the zone which contains it appear not to have diminished since the time of Ptolemy? This difficulty truly contains nothing important: what means have we of knowing in what state the Milky Way was 1500 years ago?

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* I purposely put aside the idea of the Brahmin philosophers, that there exists, besides the four terrestrial elements, a *fifth element*, the *Akasch*, of which the heaven and stars are formed. The Akasch may undoubtedly be legitimately likened to the nebulous matter of modern astronomers; but nothing, I believe, would authorize the supposition that the Indians imagined that new stars were engendered, in our own times, and under our eyes, at use of the Akasch.

*Of the condensation the diffused matter must undergo in order to be transformed into Stars.*—The opponents of the great ideas I have referred to, seem to have entered upon a more serious field of objections, when, founding their opinion on the excessive rarity of the diffused matter, they *assure* us that the whole of this matter observed in all the regions of space, would not compose a star comparable to our sun in size and density. A calculation of Herschel's has reduced the difficulty to its true value.

Let us take a cubical agglomeration of nebulous matter, the side of which, seen from the earth, subtends only an angle of ten minutes. Let us suppose that this agglomeration is situated in the region of stars of the eighth or ninth magnitude. The calculation will shew, that its volume will rise to more than two trillions of times that of the sun. This result may be put in this other form: the diffused matter contained in the cube of 10′ the side, after having been condensed more than two trillions of times, would still occupy as large a volume as our sun. Now, have these objectors reflected on the condensation expressed by the prodigious number of two trillions? The objections against the actual production of stars, founded on the rarity of the diffused matter, may therefore be set entirely aside.

*Comparative intensities of the total light of a Nebula, and the condensed light of a Star.*—After having examined the questions of volume and density, it ought to be asked if the feeble scattered light of a nebula would be sufficient to produce, by means of concentration, the lively, penetrating, scintillating light of a star?

Herschel, I believe, never studied the problem in this light. But, if I am not mistaken, it may be illustrated in a few words.

Nothing being first established in principle, I hasten to remark, that the condensation of the diffused matter *does not increase the luminous properties* of each of the molecules. But I set entirely aside this possibility of increase of splendour, and reduce the question to very simple terms; are the feeble lights spread over all the points of such or such a diffused nebula, equal *in the sum*, to the light of such or such a star?

There are no practicable experimental means of conveniently uniting in a single point, the light emanating from the whole superficial extent of a great nebula. The inverse operation is, on the contrary, easy. If we gradually withdraw the glass of a telescope from the place which it occupies when the vision is distinct, we see the image of each star successively *enlarge* and lose its intensity. In displaying one of these images in this manner, till we make it fill nearly the whole field of vision, we make it at last not more brilliant than the milky nebula. This once obtained, calculations into which various elements enter, as well as various corrections of which I cannot give a complete enumeration without exceeding the limits imposed on me, lead to the results sought for: I may say to the numerical approximations which exist between the intensities of the total lights dispersed over a great extent of milky nebula, and the concentrated light of stars. The result of these experiments and calculations strengthens the ideas of Tycho, Kepler, and Herschel, on the transformation of nebulæ into stars.

*Changes observed in certain Nebulæ.*—By comparing the observations of the years 1780 and 1783, with those of 1811, Herschel found that the nebula of Orion had sensibly changed both in form and extent. This was, according to the expression of Fontinelle, *to have caught Nature in the fact.*

Boulliaud, Kirch, and Le Gentil, believed, as early as 1667, 1678, and 1759, that the nebula of Andromeda underwent great variations. Mairan says the same thing of the nebula of Orion, and supports his statement by the authority of Godin and Fouchy; astronomers, nevertheless, continued in uncertainty. They remarked, not without reason, that, in order to be in every respect comparable, observations on objects of such small brilliancy, and so ill defined, ought to be made at all the different periods with glasses of the same power; but this condition had not been attended to. Herschel, on the contrary, strictly conformed to it. His telescope, in 1811, in no respect differed from the instrument of 1783. This gave him the confidence to say: I have *proved* these changes, (*Phil. Trans.*, 1811, p. 324). The *proof* did not appear so indisputable as to prevent the son of Sir William from recently ranking himself among the sceptics. John Herschel's beautiful memoir is too much out of the plan I have chalked out for myself, to permit me to analyze it in this place.

*Planetary Nebulæ. Is it true that, in order to explain the uniform luminosity of their discs, it is indispensably necessary to suppose that the diffused matter is opaque after it reaches a certain degree of concentration?*—Herschel applied the above name to nebulæ which resemble the planets of our system in form. They are circular or slightly elliptical; some have their contours distinctly defined; others appear surrounded by a slight nebulosity; their light is equally bright over the whole extent of the disc. Among the planetary nebulæ discovered by Herschel, I find some of ten, fifteen, thirty, and even sixty seconds in diameter.

Herschel regarded the physical constitution of planetary nebulæ as very problematical. His fertile imagination could furnish him with nothing very plausible or satisfactory on this subject. These bodies could not be likened to the globular nebulæ composed of stars, without explaining why their light did not present any increase of intensity towards the centre. To transform the planetary nebulæ into stars, properly so called, was to disregard all analogy; it was to create stars with actual diameters thirteen thousand times greater than the diameter of the sun (diameters of 4600 millions of leagues), and to ascribe to stars a kind of dull light which no star has hitherto exhibited.

After much hesitation, Herschel decided on considering the planetary nebulæ as agglomerations, *already very much condensed*, of the diffused matter. This assimilation, it cannot be disguised, demands a hypothesis which appears not very natural. In order to explain why the lustre of nebulous planetary discs is not much stronger in the centre than towards the edges, it is necessary to admit that the light does not come from the whole depth of the nebula (otherwise its intensity would increase with the number of material and radiating particles contained in the direction of each visual ray); it is necessary to reduce the radiation to the state of being purely superficial; we must grant, in other words, that when it attains a certain density, the diffused milky matter, as one would call it, *ceases to be diaphanous.*

I do not know, but it seems to me, that all these suppositions may be avoided by admitting that these planetary nebulæ are *nebulous stars*, so remote from the earth that the central *star* no longer predominates by its splendour over the diffused luminosity with which it is surrounded. It would be superfluous to repeat here what I have already said in another part of this essay.

I add a single word on the danger that would arise from drawing too absolute consequences from the evolutions of the diffused matter, and the various forms it may assume when agglomerating. Has it not been alleged but lately, that, in the nebula of Orion, the milky substance is not in immediate contact with the stars of the celebrated trapezium so well known to all astronomers? Has it not been said that these stars are, as it were, isolated in the midst of the nebulosity, and that a dark space surrounds them? Astronomers, I admit, have not yet *demonstrated* that we ought to see, in the phenomenon of which I have spoken, any thing else than a simple effect of contrast; nothing proves that it is any thing else than a very feeble light becoming effaced by the contact of a more brilliant one. To remove all doubts, it is necessary to throw, by means of the reflection of a flat diaphanous mirror with parallel faces, placed before the object-glass or the aperture of a telescope, the image of some star on the image of the nebula, and observe if the image of the star thus reflected shall seem likewise surrounded with a dark space. In the mean time, every thing authorizes us to suppose that the milky molecules are subjected, in the vast regions of space, to forces of which we have r [illegible]. The observers who have followed the prodigi[illegible] en almost instantaneous, changes of Halley's comet in its last appearance, will not gainsay me; the reserve I recommend will appear to them, I hope, quite natural.

*Diffused cosmic matter, not luminous of itself, and imperfectly diaphanous.*—Herschel thinks that he has determined, by the observations I am about to mention, that besides the diffused matter, luminous of itself, of which we have spoken so much, there exists in space another equally diffused, but not radiating, and imperfectly diaphanous.

In March 1774, this celebrated astronomer perceived on the north of the great and beautiful nebula of Orion, on both sides of the celebrated nebulous star signalized by Mairan, two other smaller stars surrounded in the same manner with circular nebulosities.

In the month of December 1810, the nebulosities of these two small stars were dissipated. On the 19th January 1811, no trace of them was to be seen, even with a telescope of 39 feet. With regard to the nebulosity of the principal star, it had undergone no change save becoming very much weaker.

Herschel believed that the three nebulosities in question were not real. When a star is seen through a mist, it appears to be in the centre of a luminous glory. This glory is composed of a portion of the mist illuminated by the star. An analogous cause produced, according to this illustrious astronomer, the nebulosities observed in 1774 around the three stars mentioned; only, the ordinary mist was replaced by a cosmic matter, nearer to us than the three stars, situated, however, in the high regions of the firmament, and in immediate connection with the great nebula of Orion. The matter did not shine with its own light, since, at a certain distance from the three stars, no trace of it was seen. It reflected strongly towards our eye the starry rays which traversed it, under incidences very little removed from the perpendicular; it wanted that extreme diaphaneity which our fancy takes pleasure in conferring on gaseous matters situated in the celestial spaces; finally, it was by obeying a clustering power, which all the nebulous matter of Huygens is subject to, that it ceased in 1810 to interpose itself exactly between the two small stars and us, and thus it happened that the phenomenon so visible in 1774 no longer existed 36 years after.[*]

Such is Herschel's theory, if I understand it aright. I shall not here consider whether it might not have been more simple to assimilate the circular nebulosities of the three stars of Orion to the luminous atmospheres of ordinary nebulous stars, than to attribute the weakened light of the largest, and the disappearance of the two others, to a motion of the atmospheres towards the centre of each star. I see nothing in the observation which, at first sight, would oppose this mode of explanation; but the strictest caution is a duty whenever we differ from the opinions professed by the illustrious astronomer of Slough.

## MILKY WAY.

*Opinions of the Ancients on the Milky Way.*—Such is the name applied to the luminous whitish zone which every one has remarked in the starry sphere. Every one also knows that this zone goes round the whole firmament; that it very nearly traces one of its great circles, not, however, without undergoing a sharp bifurcation from which results a secondary bow, which, after continuing separated from the principal arc for the extent of about 120°, again becomes confounded with it.[*]

The Milky Way excited the eager attention of the ancient philosophers. Manilius describes at length, in his poem, constellations which it traverses. He likewise makes acquainted with the greater number of the explanations which have been given of this imposing phenomenon. These fruits of Grecian fancy, and such as it may be possible to collect from the other writers of antiquity, do not deserve, in the present day, the honour of a serious examination. Of what importance is it to science—I might almost say of what importance is it to the history of science—that Aristotle has said of the Milky Way, "that it is a luminous meteor, situated in the *middle region?*" Does any one desire to know that they have gone the length of seeking for the origin of this immense whitish girdle in the drops of milk which the infant Hercules let fall from the breast of Juno;* and in the burnt track which was left behind by the chariot of Phæton, or by some star suddenly darting, in former times, from its ordinary place, and shooting across space? Must we remind the reader that Œnopides and Metrodorus believed that the Milky Way is the route which the sun anciently abandoned, as it approached its present zodiacal course, and to which it was confined a sufficient length of time to leave indelible marks of its passage? From the time that comets have irretrievably broken in pieces the solid spheres to which the ancients attributed such an important part in the mechanism of the universe, no more attention has been paid to an often cited passage of Macrobius; a passage in which this author informs us that Theophrastus regarded the Milky Way as the line where the two hemispheres, which, according to him, compose the celestial vault, are united or solde together. The extravagance and absurdity of these conc tions is a reason for giving more prominence to a though Democritus, again brought forward and illustrated by nilius, which presents so much that is subtile, ingenious, difficult to discover. According to these philosophers, if Milky Way shines with a lively lustre, it is because the s in it are too close upon each other for us to see them, c sidering their prodigious distance, one by one ; it is beca the images of so many stars greatly condensed are confoun with each other.

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[*] The authenticated disappearance of a starry nebulosity would be a very extraordinary phenomenon and very fruitful in results. I have, therefore, thought it requisite to inquire whether the annals of science offer any fact analogous to the two cited by Herschel. My search has not been, in my opinion, unfruitful. Lacaille, during his residence at the Cape, saw in the constellation Argo (310 Bode) five small stars in the centre of a nebulosity, of which Mr Dunlop, with much better instruments, could perceive no traces in 1825.

[†] The breadth of the Milky Way seems very unequal. In some places it does not exceed 5°; in others the breadth is 10° and even 16.° *Its two* branches, between Serpentarius and Antinous, retire more than 22° of the sphere.

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* When the great Condé confined himself to milk as his sole nourishment, a poet of the day, enumerated in Latin verse the true or imaginary properties of the precious liquid. Fontenelle translated the piece of P. Commire. I shall here quote the verses relating to the Milky Way.

Voyez ces astres dont à peine
Il parvient jusqu' à nous un faible lueur:
C'est là ce même lait qui tomba par malheur
De la bouche du fils d'Alcmène:
Et comme il eût été perdu,
Jupiter ménagea ces précieuses gouttes:
En astres il les changea toutes,
Et du Chemin de Lait voilà ce qu'on a su.

*Opinions of the Moderns : Galileo, Wright, Kant, Lamber* As soon as he directed one of his earliest telescopes towa the heavens, Galileo discovered multitudes of new stars. T sixth magnitude ceased to be the last limit of visibility. T belt and sword of Orion, in which the Greek and Arab astronomers could count only eight of these stars, exhibited him upwards of eighty. The Pleiades exhibited thirty-six him, instead of the six or seven of the ancients. The Mi Way presented distinct stars, where nothing before had e been seen but confused lights. Thus, Galileo again reviv the explanation of Democritus ; but supporting it by prec observations, he brought it out, to a certain point, from t domain of mere conjecture. Ever since it has been alm generally adopted.

The explanation of Democritus and Manilius left entir aside circumstances, not less worthy of the attention of ast nomers than are the light and whiteness of the Milky Wa I speak of the *form* of the phenomenon, its continuity, and t almost perfect coincidence of its principal branch with c of the great circles of the sphere. A coincidence so sin lar, a continuity so astonishing, cannot be the effect of chan these are two things which cannot but have physical caus The investigation, the profound study of these causes, see to have been a predominating object with Herschel. It is the form, in the position of the Milky Way, considered ways as an agglomeration of stars, that the illustrious ast nomer conceived that he had discovered the secret of the c str[illegible] of the heavens.

Before analyzing the immense labours of Herschel relative to the Milky Way, I ought to draw attention to the fact that three *thinkers*, if not three *observers*, had preceded him in this career; these are *Wright* of Durham, *Kant*, and *Lambert*. A few words will be sufficient to shew that these three names do not deserve the oblivion into which it has been the custom to let them fall.

I have been unable to procure Wright's memoir, and know not even the title of it;* but I find at the date of 1755, in Kant's *Theory of the Heavens*, that the Durham savant rejected all idea of a fortuitous and confused dispersion of stars, as irreconcileable with the appearance of the Milky Way; that its aspect, on the contrary, led him "to admit a systematic disposition of the stars around a ground plane."

Kant, in accordance with the quotation just given, completes Wright's idea. He observes that the plane on both sides of which the stars are grouped, must necessarily pass by the earth. "In admitting," he adds, "that the stars are nearer the plane in question than the other regions of space, our eye, in plunging into the starry plain, would believe that it perceives on the contour of the apparent vault of the firmament, the *whole* of the stars near the plane; they will there form a zone which will be distinguished from the rest of the heavens by a greater luminous intensity. This zone of light will extend itself in a great circle, since the eye of the observer is supposed to be in .the plane itself of the stratum of stars. The stars, finally, being very small and very numerous, will not be distinguishable one from another; they will produce a confused light, of a uniform whitish colour; in other words, a milky way.

Kant was well aware that, in his hypothesis, the appearances of the starry heavens ought, to a certain point, to present something gradual. Thus he adds: "The regions not comprised in the whitish track of the Milky Way, are the richer in stars the nearer they approach the centre of that track; the greater part of the 2000 stars discernible in the firmament by the naked eye, is included in a zone not very broad, of which the Milky Way occupies the centre."

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* It has occurred to me at this moment to consult the recently printed catalogue of the library of the Royal Society of London, and I find the following: 'WRIGHT (Thomas) *Clavis cœlestis;* being the explication of a diagram entituled, A Synopsis of the Universe, or the Visible World Epitomized, 4to. London, 1742.' I do not know whether it is this book or that which I find indicated in Lalande's biography, under the title, *The Theory of the Universe*, which Kant has cited. Both of them are anterior to the work of the astronomer of Kœnigsberg.

Kant condensed his ideas in the fewest words possible, when he called the Milky Way "*the world of worlds.*"

We likewise find an explanation of the Milky Way, in the *Cosmological Letters* published at Leipsic in 1761. From the contemplation of the heavens, Lambert came to the following conclusions: The system of the stars is not spherical: the stars, on the contrary, are arranged nearly in a uniform manner between two planes extending in every direction, and comparatively near each other; our sun occupies a region but little remote from the immense stratum of stars. This is almost exactly the whole of the hypotheses adopted by Kant in his *History of the Heavens.* How has it happened that six years after the publication of this work, Lambert has made no mention of the views developed in it? And how is it that, 29 years later, Herschel, when addressing himself to the same problems, never allowed the name of the philosopher of Kœnigsberg, or of the geometrician of Mulhouse, to drop from his pen? These are two questions which I cannot answer.

*Herschel's labours on the Milky Way.*—I hasten to take up the minute analysis which Herschel substituted for the imperfect sketches of his predecessors.

We have perceived that the brilliant zone, the physical cause of which the great observer wished to discover, may have nothing real in it. It has been shewn that it is very possible that it may be only a deceptive appearance, a simple effect of projection. It was not enough, therefore, to enumerate the stars in the regions alone where they appear most condensed; it was necessary to enquire if, in gradually retiring from these regions, their number diminished with regularity or without rule. Such a labour seemed to demand the united efforts of many generations of astronomers. Herschel, however, executed it alone, and in a few years, at least as far as the question of the Milky Way required. The method he followed has acquired great celebrity from its results. It was, besides, very simple, and consisted, according to the picturesque expression of the illustrious author, in *gauging the heavens.*

In order to determine the comparative mean richness in stars of any two regions of the firmament, the observer made use of a telescope whose field embraced a circle of fifteen minutes diameter. Towards the middle of the first of these regions, he counted successively the number of stars included in ten fields contiguous, or at least, very near each other. He added these numbers, and divided the sum by 10. The quotient was the mean richness of the region explored. The same operation, the same numerical calculation, gave him an analogous result for the second region. When this last result was double, triple,—decuple the first, he legitimately deduced the consequence from it, that in an equal extent, one of these regions contained twice, three times, or ten times more stars than the other; that it presented a condensation, a degree of richness, double, triple, decuple.

The gauging tables, or soundings of the firmament, which form part of a memoir printed in 1785, in the 75th vol. of the Phil. Trans., present regions where the mean number of stars embraced in the field of Herschel's telescope was only 5, 4, 3, 2, and 1. We even find some among which at least *four* successive fields were required to meet with *three* stars. Elsewhere, on the contrary, these fields, although so restricted,—these circular areas of 15′ diameter,—contained 300, 400, 500, and even 588 stars! When the telescope was directed towards the most thickly *peopled* regions, the eye, applied to the glass, saw, in the short interval of a quarter of an hour, 116,000, stars! These numerical results are truly *prodigious.* The word prodigious, in relation to the number 116,000, will seem no exaggeration to any one who knows that the stars visible to the naked eye throughout the whole nights of the year, do not exceed about 5000, and that the ancients were acquainted with only 1022. The word will appear equally natural if we apply it to the 400, 500, and 600 stars seen simultaneously in the telescope, provided it be kept in mind, that, with a diameter of 15′, the field of the instrument embraced only a fourth part of the apparent surface of the sun.

The general aspect of the Milky Way, its form, and starry composition, deduced from telescopic observations, are explained very simply, by supposing, with Herschel, that millions of stars, nearly at equal distances from each other, form a layer or stratum, comprised between two even surfaces, parallel to, and near each other, but prolonged to immense distances; that the stratum is thus very thin, compared with the immense distances to which the two even surfaces which contain it extend in every direction; that our sun,—that the star around which the earth revolves, and from which it does not far recede,—is one of the stars composing this stratum; that we occupy very nearly the centre of it, both relatively to its thickness and to all its other dimensions. These suppositions once admitted, it will be easily understood, that a visual ray, turned in the direction of the immense dimensions of the stratum, will there encounter everywhere a multitude of stars, or, at least, that it will pass so near them that they will seem to touch each other; that, in the direction of its thickness, on the contrary, the number of visible stars will be comparatively smaller, and precisely in the relation of half the thickness to the other dimensions of the stratum; that, in the passage of the visual lines coincident with the extended dimensions, to the transverse directions, there will be, in this respect, a sudden change; that the greatest dimensions of the stratum will thus be found indicated, or, as it were, delineated on the firmament by an apparent condensation of stars, by a maximum of manifest light, and a milky aspect; finally, that the maximum of light will *appear* to be a great circle of the celestial sphere, since the earth may be considered as the centre of this sphere,—since the stratum is one of its diametral planes,—and that every diametral plane of a sphere, every plane passing by its centre, necessarily divides it into two equal parts, or, what is the same thing, cuts it according to one of its great circles. The secondary arc, detached from the principal arc of the Milky Way, towards Cepheus and Cassiopea, and rejoining it between Scorpio and Sagittarius, discloses the existence of a stratum of stars forming a small angle with the principal stratum, and again meeting it near the region which the earth occupies, and not extending beyond.

In short, if we see a much greater number of stars in certain directions than in others; if the regions with thickly placed stars form one of the great circles of the sphere; if the principal arc is double for an extent of 120°,—it is because we are plunged in a group of excessive extent and comparatively very thin; because we occupy very nearly the centre of it; and because a second group of the same form meets the first towards the region where our sun, and consequently the earth, are situated.

If we suppose that the stars of the Milky Way, *taking them altogether*, are uniformly distributed throughout all the regions of this nebula; if we admit, moreover, that the observer gauges this curious portion of the heavens with an instrument, whose power permits him to reach, in every direction, the last limits of the *starry stratum*, the number of stars contained in the visual field of the telescope will be, in each observation, so intimately connected with the length of the line comprised between the eye of the astronomer and the terminal limit of the stratum, that one of these quantities may always be deduced, by calculation, from the other. Herschel having gauged our nebula, and having estimated, as I have mentioned above, its riches in stars in all directions, was therefore in a condition to deduce therefrom the corresponding linear dimensions. The table included in his memoir of 1785, gives the distance from the earth *to the limits of the Milky Way*, that from the earth to Sirius being regarded as unity.

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|---|---|---|
| When the field of the telescope includes | *one* star, the distance in question is | 58 |
| | 10 stars | 127 |
| | 20 | 160 |
| | 50 | 218 |
| | 100 | 275 |
| | 200 | 347 |
| | 300 | 397 |
| | 400 | 437 |
| | 500 | 471 |
| | 600 | 500 |

Without, therefore, going beyond the limits of direct observations, the nebula is thus found to be *a hundred times* more extensive in one direction than in another. The numbers which I have given are those which the scrupulous observer has himself made use of to give a section, and even a figure, under three dimensions, of the vast nebula, in which our Sun figures as an insignificant star, and the Earth as an imperceptible grain of dust.

*Will the Milky Way endure for ever in the form in which we now see it? Does it not begin to shew symptoms of dislocation and dissolution?*—Herschel has clearly established, by thousands upon thousands of observations, that the whiteness of the Milky Way proceeds, *in the greater part*, from agglomerations of stars, too small and too feeble to be distinguished separately. The diffused matter, mingled in certain proportions with the stars, here plays a part as in many resolvable nebulæ; but it is evidently a *secondary* part.

Almost in every instance in which stars placed near each other are presented to our view without the apparent limits of the Milky Way, we have perceived that they tend to group themselves around many centres; that they seem to obey, like the various bodies of our solar system, an attractive force; that this force, in fine, has already produced, in certain rounded groups, very considerable effects and concentrations. Why should the stars of this great nebula, of which we form a part, escape this kind of action more than the others? If formerly they were uniformly distributed, this state must cease, and approach its termination, more and more every day. Facts confirm the results of reasoning. The stars, far from appearing uniformly distributed over the whole extent of the Milky Way, have presented to Herschel, armed with his telescopes, 157 distinct and circumscribed groups, which have taken their place in the catalogue of nebulæ, without reckoning eighteen analogous groups situated on the edge of this same zone.

Any one who examines with his eye, during a dark and very clear night, the portion of the Milky Way comprised between Sagittarius and Perseus, may remark in it eighteen regions perfectly characterized by the particular brilliancy of their light.

I shall here mention a few of these:

There exists — *a very brilliant spot under the arrow of Sagittarius.*
here is — *a very brilliant one in the shield of Sobieski.*
We perceive— *a brilliant one* to the north and a little to the west of the *three stars of Aquila.*
We notice—— *one* long and feeble which follows *the shoulder of Ophiuchus.*
We remark — *three* brilliant ones near the stars α, β, and γ of *Cygnus.*
We distinguish *three* towards and within *Cassiopea.*
There is——— a very brilliant *one* in the hilt of Perseus' Sword.
(Between α and γ of Cassiopea, there exists a very obscure place.)

No portion of the Milky Way resolvable by the telescope, has exhibited to Herschel more manifest indications, and on a larger scale, of the clustering power of stars, than the space which separates β and γ of *Cygnus.* By gauging this space, according to the method already described, for a breadth of about 5 degrees, Herschel found that 331 thousands of stars might be counted in it. This immense group already presents a kind of division ; 165 thousand stars appear to proceed to one side, and 165 thousand to the other.

Everything, therefore, justifies the opinion of this illustrious astronomer. In the series of ages, the clustering power will inevitably bring on the fracture, rupture, and dislocation of the Milky Way.

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## *Some Remarks on the Ancient Peruvians.* By SAMUEL GEORGE MORTON, M. D.*

IN my work on American skulls (Crania Americana), I have expressed the opinion that the heads of the ancient Peruvians were *naturally* very much elongated; and that they differed in this respect from those of the Inca Peruvians, and other surrounding nations; and having given this opinion at a meeting of the Academy prior to the publication of my work, I take the present occasion to renounce it.

In the American Journal of Science, for March 1840, I have already, in a brief note, adverted to this change of opinion ; and I now repeat my matured conclusions in connection with positive facts, derived from the work of a distinguished traveller and naturalist, M. Alcide D'Orbigny.

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* Communicated to the Academy of Natural Sciences of Philadelphia.

This gentleman not only visited the elevated table-land of the Andes, which was once inhabited by the ancient Peruvians, but he remained a long time in that interesting region, and has collected numerous facts in relation to the people themselves.

1. The descendants of the ancient Peruvians yet inhabit the land of their ancestors, and bear the name of Aymaras, which was probably their primitive designation.

2. The modern Aymaras resemble the surrounding Quichua or Peruvian nations in colour, figure, features, expression, shape of the head (which they have ceased to mould into artificial forms), and in fact in every thing that relates to physical conformation and social customs: their languages differ, but even here there is a resemblance which proves a common origin.

3. On examining the tombs of the ancient Aymaras, in the environs of the lake Titicaca, M. D'Orbigny remarked that those which contained the compressed and elongated skulls, contained also a greater number that were not flattened; whence he infers that the deformity was not natural, or characteristic of the nation, but the result of mechanical compression.

4. It was also remarked that those skulls which were flattened were uniformly those of men, while the heads of the women always retained the natural shape,—the squared or spheroidal form which is characteristic of the American race, and especially of the Peruvians.

5. The most elongated heads were found in the largest and finest tombs; shewing that the deformity was a mark of distinction among these people.

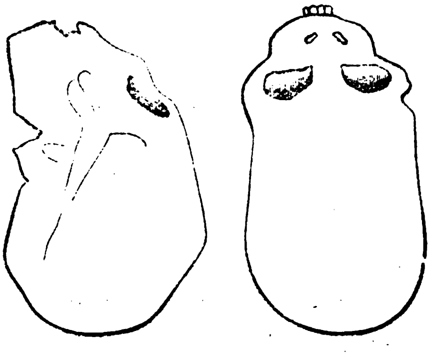
6. The researches of M. D'Orbigny confirm the statements made at distant intervals of time by Pedro de Cieza, Garcilaso de la Vega, and Mr Pentland, and prove conclusively, what I have never doubted, that these people were the architects of their own tombs and temples; and not, as some suppose, intruders who had usurped the civilization, and appropriated the ingenuity of an antecedent and more intellectual race.

M. D'Orbigny found temples from 100 to 200 metres in length, facing the east, and ornamented with rows of angular columns; enormous gateways made of a single mass of rock, and covered with bas reliefs; colossal statues of basalt; and large square tombs, wholly above ground, and in such numbers that they are compared to towns and villages.

My published observations go to shew that the internal capacity of the cranium, as indicative of the size of the brain, is nearly the same in the ancient and modern Peruvians, viz., about seventy-six cubic inches—a smallness of size which is without a parallel among existing nations, excepting only the Hindoos.

M. D'Orbigny even supposes the ancient Peruvians to have been the lineal progenitors of the Inca family; a question which is not yet decided. Supposing this to be the fact, we may inquire how it happens that the Incas should have so entirely abandoned the practice of distorting the cranium; especially as this, among the Aymaras, was an aristocratic privilege?

I was at first at a loss to imagine how this singular elongation of the head was effected; for when pressure is applied to a spheroidal head, as in the instance of the Chenouks and other tribes of the Columbia river, the skull expands *laterally* in proportion as it is depressed above; whereas, in these people, the head is narrow from the face to the occiput. It seems probable that this conformation was produced by placing splints or compresses on each side of the head from the cheek bones to the parietal protuberances, and another on the forehead, and confining them by rotary bandages. In this way the face, in the process of growth, would be protruded in front, and the head elongated backwards; while the skull,



in all other directions, could expand comparatively little. These remarks will be more readily understood by reference to the annexed outlines, which are taken from a cast of one of the skulls obtained by Mr Pentland.

Dr Goddard has suggested to me that the deformity observable in this series of crania, might have been produced by the action of rotary bandages alone without the use of splints or compresses. I admit the possibility of this result in some of the heads, but think that in others there is satisfactory evidence of the use of the splint or compress, especially on the os frontis.

I have in my possession six casts of heads and three skulls of these people, all of which present the peculiarly elongated form in question.

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## *Professor Forbes' Account of his recent Observations on Glaciers.* Communicated in the following Letters to the Editor, Professor Jameson.

COURMAYEUR, PIEDMONT, *4th July* 1842.

My Dear Sir,—Knowing that you will be glad to hear of my safe arrival amongst the Alps, and of my farther proceedings, I hasten to give you an account, in a few words, of what I have as yet done. Finding the season more than usually advanced, I hastened to reach Chamouni, in order to ascertain whether the Mer de Glace was as yet accessible in all its extent; and I arrived at the Montanvert on the 24th June, and remained there for a week. I was fortunate enough to convey all my instruments to their destination, without, I believe, injury to any one of them. The Mer de Glace, so continually visited by the curious, but so little studied, seemed to me to offer great advantages for the prosecution of the objects which I proposed to myself. At first sight it appeared to me steeper and more crevassed than I recollected it to be, and I doubted for a moment whether it was adapted for my experiments; but that doubt vanished upon closer examination; and in the course of the single week which I have been able to spend there, being favoured by most excellent weather, I have obtained results so far definite and satisfactory, that, imperfect as they necessarily are, and only the commencement of what

I expect to accomplish during the remainder of the season, I will state them shortly.

You will recollect that, in my lectures on glaciers delivered last December and January, and afterwards in an article written by me in the Edinburgh Review, I insisted on the importance of considering the mechanism of glaciers as a question of pure physics, and of obtaining precise and quantitative measures as the only basis of accurate induction. I pointed out, also, the several experiments of a critical kind which might be made; such, for instance, as the determination of the motion of the ice at different points of its length, in order to distinguish between the theories of De Saussure and De Charpentier; for, if the glacier merely slides, the velocity of all its points ought (in the simplest case) to be the same; if the glacier swells in all its mass, the velocity of the inferior part ought to be greatest. Of course, I do not now advert to the many causes which might accidentally invert this law, and which would require to be fully taken into account; still less do I mean to say that any thing I have now to state can be considered as critically decisive between rival theories; but my experiments certainly do shew that the kind of precision which I desired to see introduced into reasonings about this subject, is practically attainable, even in a far higher degree than I expected.

For example:—The motion of glaciers by the measurement of the distance of blocks upon its surface from a fixed point, from one year to another, has marked indubitably the annual progress of the ice. I do not know that any one has attempted to perform the measurement in a manner which could lead to any certain conclusion respecting the motion of the ice at one season compared with another, or from month to month; still less has any one been able to state, *with precision*, whether the glacier moves by starts and irregularly (as we should certainly expect on the sliding theory), or uniformly and evenly; and if so, whether it moves only at one part of the twenty-four hours, and stands still during the remainder (as we should expect on the dilatation theory, as commonly expounded). Now, I have already been able—

1*st*, To shew and measure the glacier motion not only from *day to day*, but from *hour to hour;* so that I can tell nearly what o'clock it is by the glacier index. That you may have an idea of the coincidence which these experiments present, I give you the longitudinal motion of a point on the Mer de Glace during four consecutive days.

15.2 inches. 16.3 inches. 17.5 inches. 17.4 inches.

2*d*, This motion, evidently incompatible with sudden starts, takes place in the glacier *as a whole*, undisturbed by the most enormous dislocations of its surface, for these measures were taken where the glacier was excessively crevassed.

3*d*, This motion goes on *day and night*, and if not with absolute uniformity, at least without any considerable anomaly. On the 28th–29th June the motion

| | | | |
|---|---|---|---|
| | from 6 P. M. to 6 A. M. | was | 8.0 inches, |
| | ... 6 A. M. to 6 P. M. | ... | 9.5 ... |
| 29th–30th, | ... 6 P. M. to 6 A. M. | ... | 8.5 ... |
| | ... 6 A. M. to 6 P. M. | ... | 8.9 ... |

seeming to shew a greater motion during the day.

4*th*, In the particular case of the Mer de Glace, the higher part (the Glacier de Lechaud) moves *slower* than the lower part near the Montanvert in the proportion of 3 to 5.

5*th*, The central part of the glacier moves faster than the edges in a very considerable proportion; quite contrary to the opinion generally entertained.

There cannot be a doubt of the accuracy of these results *within the limits* in which the experiment has been made. They prove how completely problems of a purely physical character admit of accurate investigation; and when a larger induction shall have freed the results from local errors, it is evident that we shall have the solid foundations of a theory. My wish to see the total eclipse of the sun on the 8th, has brought me to the south side of the Alps sooner than I could have wished; but I have now fixed so many points on the Mer de Glace, that, on my return thither, I shall be able to obtain more comprehensive results. But what is most important in the whole matter is this,—that an observer furnished with the proper instruments and methods may, by

nding a few days on a glacier, determine at any particular
son the amount of its motion at all the essential points,
hin the limits which any glacier theory can require.

CHAMOUNI, 10*th August* 1842.

My Dear Sir,—Since I last wrote to you on the 4th July
n Courmayeur, I have examined, in detail, the two princi-
glaciers of the Allée Blanche; and having re-crossed the
s from Courmayeur by the Col du Geant, where I had the
sfaction of still finding the remains of Saussure's Cabane of
8, I have pursued for a fortnight my experiments on the
ion of the Mer de Glace. Being composed, as you know,
everal tributaries which are in some degree independent,
presenting also a considerable variety of surface, this
ier seems as proper as any for detailed experiments, such
hose which I am attempting. Being about to quit this
on a tour to Monte-Rosa and the glaciers east of the
St Bernard, I wish to explain to you now in what re-
my observations differ from those formerly undertaken
e glaciers, and to mention a few results, which, of course,
as yet only partial, ought not to be considered as alto-
decisive of the truth or falsehood of any theory; still
ve it will be admitted that the facts established in my
nd which farther experience has confirmed), militate
y against some of the received opinions as to the cause
ier motion.

are aware, that, in my lectures on glaciers in December
uary last, and in an article in the Edinburgh Review
il, I insisted, and so far as I know it was for the first
the importance of considering the glacier theory as a
of mechanical physics, by which I mean that the cause
ment should be ascertained inductively from the ob-
otion, carefully and *numerically* ascertained at differ-
ts. It is because authors have considered the problem
a one to require a systematic analysis, that we
othing done in this respect; and it may be affirm-
ny disrespect to the ingenious persons who have
able causes for the movement of these masses of
solutions have been, like the astronomical theorie

of the earlier cosmogonists, based upon somewhat vague analogies with better understood phenomena, as when the analogy of magnetic attractions seemed to offer a parallel in the mechanism of the heavens in the theory of Gilbert, and that of fluid currents gave rise to the Cartesian vortices. The Newtonian theory was based upon its coincidence with the empirical laws of planetary motion. We have as yet no empirical laws of glacier motion, consequently no proper mechanical theory can as yet be adequately tested. I endeavoured to point out in my lectures how a mechanical theory might be deduced from observation, and how these observations might be practically made. I believe that I have also obtained for the first time, the numbers on whose importance I insisted. I am not aware that any one had hitherto proposed to determine the diurnal velocity of a given point of a glacier with reference to three co-ordinates. The analogy with the empirical laws of astronomy is both striking and just; an exact acquaintance with the path described by any molecule of a glacier, will almost as certainly lead to a knowledge of the cause of its motion, as the theory of gravitation sprung from the three laws of Kepler. We have to deal, indeed, with an effect more complex and varied; but the results contained in my last letter, already shew how much of numerical precision may be attained. I have already determined the diurnal motion of 10 points of the Mer de Glace with a probable error, not exceeding, I think, a quarter of an inch in any case; and when these observations shall have been pursued, as I expect to do, until the end of September, there will be a tolerable basis for sound speculation.

In particular, you will recollect that I pointed out last winter two experiments for distinguishing between the prevailing theories of De Saussure and De Charpentier, those of gravitation and of dilatation. One was the exact measurement of a space along the ice to be measured after a certain time, in order to ascertain whether any expansion had occurred. The other was the determination of the linear velocity of the glacier at any point, which, on the theory of Saussure, ought (if the glacier be of nearly uniform section) to be uniform throughout; on the theory of Charpentier it ought to increase from nothing at the upper extremity of the glacier, to a maximum at its lower end. The former experiment had, I have since learned, been suggested by Professor Studer to M. Escher last year, and attempted to be put in practice (though unsuccessfully) by the latter, on the glacier of Aletsch. Admitting Charpentier's theory, however, this dilatation would be too small to be successfully observed in a moderate time, and with the geometrical methods which the uneven and varying surface of the glacier enables us to employ; I have therefore not attempted it. The other method, in fact, embraces both ends; for if the movement of the glacier in its upper and lower part be determined (by *upper* I mean near its origin), the difference of the motions determines the dilatation or contraction of the intermediate part of the ice, and is liable to none of the great errors arising from the measurement of long distances. The observation, in the simplest and best form which I employ, resembles perfectly that of determining with the transit instrument the progress of a planet.

I have already said that my later observations confirm those which I previously communicated; any variations, indeed, arise solely from a change of circumstances or season, and not from errors of observation. (1.) The continuous imperceptible motion of the glacier is entirely confirmed; its bearing upon the sliding theory is very obvious. (2.) This motion is not by any means the same, however, from day to day and from week to week, as indeed already appeared from my first results. (3.) This variation of motion appears to be common to every part of the glacier, as well where compact and completely even, as where most fissured; nor perhaps is the variation of velocity greater in one case than in the other. (4.) From numerous observations, made in all parts of the glacier, it invariably results as before, that the centre moves faster than the sides of the ice-stream. In the lower and faster moving part of the glacier this disproportion is greatest, varying from one-third to one-half of the smaller velocity. Near the origin of the glacier it appears to be one-fourth or one-fifth of the smaller velocity. (5.) The variations of glacier motion affect the central parts most sensibly. (6.) The greatest daily motion which I have observed, nearly opposite the Montanvert, amounts to 27.1 inches. (7.) I have ascertained the velocity of motion much nearer the origin of the glacier than when I last wrote. This, which would appear to be nearly, if not quite an *experimentum crucis* between the sliding and dilatation theories, does not yield a result so favourable to the latter as I had at first supposed; for though it is undoubtedly true, as stated in my last, that the head of the glacier moves slower than the foot, the middle part moves rather slower than either, owing probably to the greater width and thickening of the ice there. This source of error from the varying section of the glacier I had fully anticipated; but still, when we push the experiment to a limit, and take the velocity very near the origin, the velocity ought to diminish, on the theory of Charpentier, with a rapidity not to be mistaken. Yet very near the head of the Glacier de Lechaud, the diurnal velocity is considerably more than a foot per day. I am far, however, from thinking that I am yet in a position to judge finally of the merits of any theory; my belief is, that both of those cited will as yet require great modification.

By insisting upon the treatment of the problem as one of pure mechanics, I am far from denying that the kind of investigations to which the glacier theorists have hitherto almost exclusively referred, are also of great value, such as those on the temperature and structure of the ice. The latter, in particular, is a sort of standing evidence of its mechanism, and, rightly understood, must lead to the most important confirmation of any mechanical theory. This you may believe I have made an object of very particular attention. I have now examined so many glaciers as to have a very clear idea of the empirical laws which that structure follows. Lately, I begin to perceive a connection between that structure and the facts of motion already cited. If these two classes of facts can be well brought into harmony with one another, we should have a very good chance of consolidating them into something like a theory. In my next letter, I will give you some account, at all events, of my observations on the subject, which are sufficiently definite, and probably also (without considering it as proved), of what seems likely enough to be its true explanation. I go to-morrow to the Great St Bernard, to meet M. Studer.—Believe me, very sincerely yours,

James Forbes.

Zermatt, North Side of Monte Rosa,
*22d August* 1842.

My Dear Sir,—I arrived here two days ago by a very interesting and unfrequented route. I mentioned in my last, that M. Studer and I had agreed to visit together the valleys eastward of the Great St Bernard. The Convent was our place of rendezvous, and we afterwards descended to Orsières, and turned into the Valley of Bagnes. Crossing the Alpine chain at the head of the valley, by the Col de Fenêtres, we went down to Valpelline on the Italian side, and ascended that valley quite to its origin. We then crossed to the western branch of the valley of Erin, by the Col de Collon or Arolla, a very striking glacier pass. Thence M. Studer went to the Val d'Anniviers, and rejoins me here by the way of Visp, whilst I ascended the other branch of the Eringer Thal from Evolena by way of the Ferpecle glacier, and crossed over the mountains to this place, by a pass higher and much longer than the Col du Géant, which presents, certainly, the grandest views I have hitherto met with in the Alps. I must not, however, stop to describe, as my present object is to fulfil the promise in my last respecting the structure of glacier ice.

The internal veined or ribboned structure presented by all glaciers in a greater or less degree, appears to be the only true essential structure which they possess, and which, you will recollect, I described in a paper printed in your Journal for January last. The existence of granules divided by capillary fissures, as well as of large crevasses, are equally unessential to glacier structure, and subordinate to the other. Whatever other result may flow from the examination of glaciers this summer, by the many persons who are probably at this moment directing their attention to them, this, I am sure, will be admitted, that the veined structure is not peculiar to some glaciers, as some would maintain, nor to some years, as has been alleged by others; but that it is perfectly general and systematic, having one general type or form, which is varied according to external mechanical circumstances. Being then the most essential and intimate part of the glacier formation, as well as one of its most obvious and universal features (especially on those glaciers which are most commonly visited), it is equally singular that it should not have been sooner noticed, or if noticed, never once alluded to by the eminent and ingenious authors who have treated of existing glaciers and their effects.

With respect to the general type or form of this structure, I am happy to say, that I have found not the slightest reason to modify the description which I have given in the paper above alluded to of the conformation of the glacier of the Rhone. The description is characteristic, not of that glacier only, but of every other, with certain modifications similar to the variation of the parameter of a curve; variations, therefore, not in kind but in degree. The most beautiful structure I have ever met with is in the glacier of La Brenva in the Allée Blanche, which was one of the earliest I examined this season, and in which I found all that I had seen, though imperfectly, on the glacier of the Rhone (which it resembles in the circumstances of being derived from an icy cascade, and in having a considerable breadth in proportion to its length), developed in a manner so clear and so geometrically precise, as gave me the most lively satisfaction. I refer to my former paper for the figure and description of that structure; I have found the same conoidal surfaces, and the same false appearance of horizontal stratification on the terminal face of the glacier, arising from the veins dipping inwards at first at an angle of only 5°, rising to 10°, 20°, up to 60° and 70°, if we follow the medial line of the glacier, or axis parallel to its length. The sides of the glacier, in like manner, have their cleavage planes or veins dipping inwards towards the centre at an angle determined by the declivity of the rock or moraine which supports them, gradually becoming more vertical as the centre of the glacier is approached, where they twist round by degrees, so as to become transverse to its length, and to form part of the system of planes dipping inwards first described. Fig. 1 exhibits a section parallel to the length. Fig 2, a transverse section.

Fig. 1.

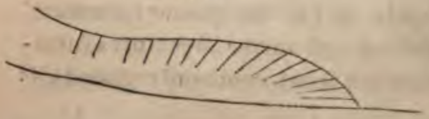
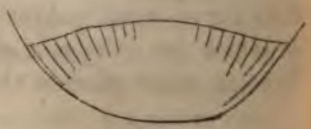


Fig. 2.



You are already aware that this structure consists in the alternation of more or less perfectly crystallized ice in parallel layers, often thinning out altogether like veins in marble, not unfrequently parallel and uniform like a ribboned calcedony or jasper.

I will, for brevity, merely state the modifications which this fundamental type undergoes, bringing together glaciers of all classes, but reserving the detail of examples and proofs, of which my experience has already furnished me with a great number, to another occasion. If a glacier lies long and narrow, as the Lower Aar, or the Mer de Glace of Chamouni, the frontal dip is the least conspicuous part of the phenomenon; and if it terminate in an icy cascade, as in the second case, it might escape observation altogether. The vertical planes parallel to the length, or nearly so, usurp nearly all the breadth of the glacier, and only in the centre is a narrow space, where not unfrequently the structure appears quite undefined. I have satisfactorily made out, however, in every glacier which I have had the means of examining with that view, that the conoidal structure, however obscured, exists in all parts of the true glacier, modified, according to its length and breadth, in the manner which figs. 3 and 4 indicate. I need not add, that these rude sketches are not intended to be considered as rigorously exact, but only to explain generally my meaning.

Fig. 3.

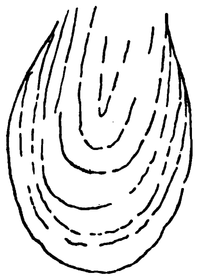
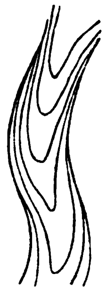


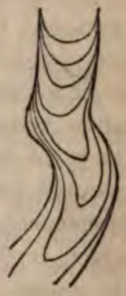
Fig. 4.



There is yet another modification, but only a modification, of the above, namely, in the case of extremely steep glaciers, but which are coherent, and not crevassed into pyramids. There are numberless examples of these in all the higher vallies of the Alps, which do not descend into the hollows, but festoon the steep sides of snowy mountains. They are, I believe, what Saussure called glaciers of the second order, and have no relation to *névés*, so far as I can attach a meaning to that term. They are of hard ice, and almost invariably present an appearance of stratification parallel to the soil on which they rest. This stratification is only apparent; the cleavage planes dip forwards and outwards, instead of dipping inwards, as in the terminal portion of glaciers of less inclination. The surfaces of crystallization have, in this case, absolutely the form of a scallop-shell, the lip or front being always inclined below the horizon. I attach importance to the community of feature in glaciers of every form and inclination, because it indicates that the origin of the structure cannot be unimportant, considering its generality; and in this particular case of small steep glaciers, it appears, I think, that M. de Charpentier, who has justly denied the stratification of glaciers in general, has wrongly admitted the existence of strata in the case in question, which he regards as formed by the intercalation of mud from the soil in some manner, which, if I recollect rightly, he does not very clearly describe. Now, these seeming strata of mud I have examined in a multitude of cases, and found invariably to result merely from the percolation of dirt from the moraine, sometimes even accompanied by small fragments of rock, into the more spongy and less crystalline veins of the glacier mass which already existed: and the proof is, that, by cutting with a hatchet, we gradually gain the pure ice, equally veined with the exterior, but not discoloured. I may observe, in passing, that the fissures which, in the lower part and near the sides of glaciers, form the granules, about which so much has been written, are stopped by the independent formation of the veins in the ice, which thus demonstrate their prior origin.

One afternoon I happened to ascend higher than usual above the level of the Mer de Glace, and was struck by the appearance of discoloured bands traversing its surface nearly in the form indicated in fig. 4. These shades, too indistinct to be noticed when near or upon the surface, except upon very careful inspection, are very striking and beautiful when seen at a distance by a light not too strong, as in the afternoon or by moonlight. They are evidently bands of dirt on the surface of the ice, having nearly the form of very elongated parabolas merging in the moraines on either side, widest apart from one another in the centre, and confounded towards the edge. For some time I was at a loss to conceive how these sort of false moraines could spread from side to side of the glacier, but I at length assured myself that it was entirely owing to the structure of the ice, which retains the dirt diffused by avalanches and the weather on those parts which are most porous, whilst the compacter portion is washed clean by the rain, so that these bands are nothing more than visible traces of the direction of the internal icy structure, and of course correspond with what has been already stated as to the forms in which the conoidal surfaces intersect the plane of the glacier. I counted distinctly sixteen of these bands on the surface of the ice then in view. I afterwards traced them to the higher part of the ice-field; and the only distinction which I there observed was, that the loops of the curves were less acute, or more nearly circular, fig. 5. All glaciers do not shew this external evidence of their structure equally, as there are some glaciers which possess the structure itself more developed than others. The cause of the dazzling whiteness of the Glacier des Bossons at Chamouni is the comparative absence of these layers of granular and compact ice; the whole is nearly of uniform consistence, the particles of rock scarcely find a lodgment, the whole is washed clean by every shower. The superficial bands are well seen on the Mer de Glace of Chamouni, and, to quote another example, one of the last I have seen, very admirably on the Glacier of Ferpecle in the Valley of Erin, where I counted above thirty in view at once.

Fig. 5.



I am quite persuaded that these bands, and of course the structure which they represent, have their origin in the movement of the glacier; and if the laws of movement, ascertained independently, shall coincide with, or confirm, the phenomena of structure, we shall be better able, from the comparison of the two classes of facts, to decide upon the *cause* of movement.

What I have hitherto stated is matter of *fact.* I will state very briefly what I am disposed to deduce by way of hypothesis.

It is impossible to consider these structural bands on the surface of the glacier, in combination with the fact established in my former letters, that the centre of the glacier moves considerably faster than its edges, without believing that the bands are an indication of the motion, and that the motion gives rise to the veined structure. These dirt-bands perfectly resemble those of froth and scum which every one has seen upon the surface of slowly-moving foul water; and their figure at once gives the idea of *fluid motion*, freest in the middle, obstructed by friction towards the sides and bottom. It will be found that the analogies are entirely favourable: the glacier struggles between a condition of fluidity and rigidity. It cannot obey the law of semifluid progression (maximum velocity at the centre, which is no hypothesis in the case of glaciers, but a fact), without a solution of continuity perpendicular to its sides. If two persons hold a sheet of paper, so as to be tense, by the four corners, and one moves two adjacent corners, whilst the other two remain at rest, or move less fast, the tendency will be to tear the paper into shreds parallel to the motion; in the glacier, the fissures thus formed are filled with percolated water which is then frozen. It accords with this view, 1. That the glacier moves fastest in the centre, and that the loop of the curves described coincides (by observation) with the line of swiftest motion. 2. That the bands are least distinct near the centre, for there the difference of velocity of two adjacent stripes parallel to the length of the glacier is nearly nothing; but near the sides, where the retardation is greatest, it is a maximum. 3. It accords with direct observation (see my last Letter), that the *difference* of velocity of the centre and sides is greatest near the lower extremity of the glacier, and that the velocity is more nearly uniform in the higher part; this corresponds to the less elongated form of the loops in the upper part of fig. 5. 4. In the highest part of such glaciers, as the curves become ess bent, the structure also vanishes. 5. In the wide saucer-shaped glaciers already spoken of, which descend from mountain-slopes, the velocity being, as in shallow rivers, nearly uniform across their breadth, no vertical structure is developed. On the other hand, the friction of the base determines an apparent stratification, parallel to the slope down which they fall. 6. It also follows immediately (assuming it as a fact very probable but still to be proved, that the deepest part of the glacier moves slower than the surface), that the *frontal dip* of the structural planes of all glaciers diminishes towards their inferior extremity, where it approaches 0, or even inclines outwards, since there the whole pressure of the semifluid mass is unsustained by any barrier, and the velocity varies (probably in a rapid progression) with the distance from the soil; whilst, nearer the origin of the glacier, the frontal dip is great, because the mass of the glacier forms a virtual barrier in advance; and the structure is comparatively indistinct for the same reason that the transverse structure is indistinct, viz. that the neighbouring horizontal prisms of ice move with nearly a common velocity. 7. Where two glaciers unite, it is a fact that the structure immediately becomes more developed. This arises from the increased velocity, as well as friction of each, due to lateral compression. 8. The veined structure invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is divided into pyramidal masses. Now, this immediately follows from our theory; for so soon as lateral cohesion is destroyed, any determinate inequality of motion ceases, each mass moves singly, and the structure disappears very gradually.

I might add more illustrations; but let these suffice for the present. It is not difficult to foresee, that, if my view should prove correct, a theory of glaciers may be formed, which, without coinciding either with that of Saussure or Charpentier, shall yet have some thing in common with both. Whether that of M. Rendu may not avail something, I am unable to say, not yet having been able to procure his work.

It yet remains to decide, what is the cause of the succession of dirt-bands at considerable distances on the surface of the glacier, indicating the succession of waves of more or less compact ice. In all the glaciers where I have yet distinctly observed them, they appear to follow a regulated order of distances, nearly the same for a considerable space, but closer the farther we ascend the glacier. I cannot help thinking that they are the *true annular rings* of the glacier, which mark its age, like those of a tree, only increasing instead of diminishing in breadth as the ice grows older, coinciding again with the fact which I formerly established, that the higher part of a glacier moves, generally speaking, more slowly than its lower extremity. The different states of the glacier at different seasons, the presence or absence of snow, or even the simple difference of velocity at different seasons, would be sufficient to account for this alternation of structure. There is no cause so likely to produce it as some *annual change*. I may add, that some observations which I have already made on the distances of these bands, as well as information which I have endeavoured to collect, lead me at least to have some doubt as to the correctness of the opinion generally entertained that the glaciers are stationary in winter, perhaps even, that there is any very great inequality in their march at different times of the year. I am, my dear Sir, yours very truly,

JAMES D. FORBES.

Professor JAMESON.

V3

## *Notes on the Effects produced by the Ancient Glaciers of Caernarvonshire, and on the Boulders transported by Floating Ice.*[*] By CHARLES DARWIN, Esq., M.A., F.R.S. and F.G.S.

GUIDED and taught by the abstract of Dr Buckland's memoir "On Diluvio-Glacial Phenomena in Snowdonia and the adjacent parts of North Wales,"[†] I visited several of the localities there noticed; and having familiarized myself with some of the appearances described, I have been enabled to make a few additional observations.

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[*] From the Philosophical Magazine, vol. xxi. No. 137.

Read before the Geological Society, 15th December 1841; and the ab- ct is published in the Athenæum, 1842, p. 42.

Dr Buckland has stated, that, a mile east of Lake Ogwyn, there occurs a series of mounds, covered with hundreds of large blocks of stone, which approach nearer to the condition of an undisturbed moraine than any other mounds of detritus noticed by him in North Wales. By ascending these mounds, it is, indeed, easy to imagine that they formed the north-western lateral moraine of a glacier, descending in a north-east line from the Great Glyder mountain. But at the southern end of Lake Idwell, the phenomena of moraines are presented, though on a much smaller scale, with perfect distinctness. On entering the wild amphitheatre in which Lake Idwell lies, some small, conical, irregular little mounds, which might easily escape attention, may be seen at the further end. The best preserved mounds lie on the west side of the great, black, perpendicular face of rock forming the southern boundary of the lake. They have been intersected in many places by streams, and they are seen to consist of earth and detritus, with great blocks of rock on their summits. They at first appear quite irregularly grouped ; but to a person ascending any one of those furthest from the precipice, they are at once seen to fall into three (with traces of a fourth), narrow, straight, linear ridges. The ridge nearest the precipice runs some way up the mountain ; but the outer one is longer and more perfect, and forms a trough with the mountain-side, from 10 to 15 feet deep. On the eastern and opposite side of the head of the lake, corresponding but less developed mounds of detritus may be seen running a little way up the mountain. It is, I think, impossible for any one who has read the description of the moraines bordering the existing glaciers in the Alps, to stand on these mounds, and for an instant to doubt that they are ancient moraines; nor is it possible to conceive any other cause which could have abruptly thrown up these long, narrow, steep mounds of unstratified detritus against the mountain-sides. The three or four linear ridges evidently mark the principal stages in the retreat of the glacier ; the outer one is the longest, and diverges most from the great wall of rock at the south end of the lake. The inner lines distinctly define the boundary of the glacier during the last stage of its existence. At this period, a small and distinct glacier descended from a narrow but lofty gorge on the north-western end of the lake; and here, remnants of a terminal moraine may be traced in the little mounds, forming a broken semicircle round a rushy plain, scarcely more than a hundred yards in diameter. The rocks are smooth, mammillated, and scored, all round the lake, and at some little depth beneath the surface of the water, as I could both see and feel. Similar marks occur at great heights on all sides, far above the limits of the moraines just described, and were produced at the time when the ice poured in a vast stream over the rocky barrier bounding the northern end of the amphitheatre of Lake Idwell. I may here mention, that about eighty yards west of the spot where the river escapes from the lake, through a low mound of detritus, probably once a terminal moraine, there is an example of a boulder broken, as described by Charpentier and Agassiz, into pieces, from falling through a crevice in the ice. The boulder now consists of four great tabular masses, two of which rest on their edges, and two have partly fallen over against a neighbouring boulder. From the distance, though small in itself, at which the four pieces are separated from each other, they must have been pitched into their present position with great force; and as the two upright thin tabular pieces are placed transversely to the gentle slope on which they stand, it is scarcely possible to conceive that they could have been rolled down from the mountain behind them; one is led, therefore, to conclude that they were dropped nearly vertically from a height into their present places.

The rocky and steep barrier over which the ice from the amphitheatre of Lake Idwell flowed into the valley of Nant Francon, presents from its summit to its very foot (between 400 and 500 feet), the most striking examples of boss or dome formed rocks; so much so, that they might have served as models for some of the plates in Agassiz' work on Glaciers. When two of the bosses stand near and are separated only by a little gorge, their steep rounded sides are generally distinctly scored with lines, slightly dipping towards the great valley in front. The summit of the bosses is comparatively seldom scored; but on one close to the bridge over the river Ogwyn, I remarked some singular zigzag scores. At this spot the cleavage of the slate is highly inclined, and owing apparently to the different degrees of hardness of the laminæ, smooth and gentle furrows have been produced by the grinding of the ice, transversely to the scores, and to the probable course of the glacier. Here, as well as in some few other places, I noticed an appearance which made it vividly clear that these bosses had been formed by some process quite different from ordinary or aqueous erosion; it is the abrupt projection from the smooth surface of a boss of a piece of rock a few yards square, and one or two feet in height, with its surface smoothed and scored like the boss on which it stands, but with its sides jagged; if a statuary were to cut a small figure out of a larger one, the abrupt projecting portions, before he quite completed his work, might be compared to these masses of rock; how it comes that the glacier, in grinding down a boss to a smaller size, should ever leave a small portion apparently untouched, I do not understand.

On the summit of some of the bosses on this barrier there are perched boulders; but this phenomenon is seen far more strikingly close to Capel Curig, where almost every dome of rock south of the Inn is surmounted by one or more large angular masses of foreign rock. The contrast between the rude form of these blocks, and the smooth mammillated domes on which they rest, struck me as one of the most remarkable effects produced by the passage of the glaciers. On the sides of the mountains above Capel Curig, I observed some boulders left sticking on very narrow shelves of rocks, and other boulders of vast size scattered in groups. The largest boulder I noticed there was about twenty-six feet in length, by twelve in breadth, and buried to an unknown thickness.

Proceeding down the great straight valley of Nant-Francon, which must formerly have conveyed the united glaciers from Lakes Idwell and Ogwyn, we continue to meet with boss-formed rocks till below the village of Bethesda.

From this point towards Bangor, these boss-formed rocks become rare: at least it is certain that a large number of hummocks of rock with rugged surfaces project, whereas *higher up in this* valley, and in all the great central valleys of Snowdonia, such unground hummocks are not to be met with. At Bethesda, unstratified masses of whitish earth, from ten to forty feet in thickness, full of boulders mostly rounded, but some angular, from one to four feet square, are first met with. This deposit is interesting from the boulders being deeply scored, like the rocks *in situ* over which a glacier has passed. The scores are sometimes irregular and crooked, but generally quite parallel, as I distinctly saw over the entire side of one large block. Some of the blocks were scored only on one side, others on two sides, but from the difficulty of turning over the larger ones, I do not know which case is most common. I saw one large block on which the scores on the opposite sides were all parallel; and another irregularly conical, four feet in length, of which three-fourths of the circumference was marked with parallel striæ, converging towards the apex. In the smaller *elongated* blocks, from six to twelve inches in diameter, I observed that the striæ were generally, if not always, parallel to their longer axis, which shews that, when subjected to the abrading force, they arranged themselves in lines of least resistance; but of three large blocks which remained imbedded in a perpendicular cliff, the vertical sides of two were scored in horizontal lines, and of the third in an oblique direction. These several facts, especially the parallel striæ on the upper and lower surfaces, shew that the boulders were not scored on the spot where they are now imbedded, as seems to have been the case with the boulders described by Mr Maclaren,* in the till near Edinburgh. The contrast is very striking in the state of the surface of these boulders, and those which lie scattered high up on the sides of the adjoining hills and of the great central valleys, or are perched on the worn bosses of naked rock; such boulders, as I particularly noticed, present no signs of scores or striæ, as might have been anticipated, if, as is supposed, they were transported on the surface of the glaciers. In the quarries which I examined, namely, below Bethesda, and at some little height on the eastern side of the village, the till rested on slate-rocks, not worn into bosses. I found, however, a rather smooth pap of greenstone marked with a few deep scores.

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* Geology of Fife and the Lothians, p. 212.

The till forms, at the height probably of 600 feet above the sea, a little plain, sloping seaward; and between Bethesda and Bangor there are other gently inclined surfaces, composed of till and stratified gravel. Considering these facts, together with the proofs of recent elevation of this coast, hereafter to be mentioned, I cannot doubt that this till was accumulated in a sloping sheet beneath the waters of the sea. In composition it resembles some of the beds of till in Terra del Fuego, which have undoubtedly had this origin. I presume the scored, rounded, and striated boulders were pushed, in the form of a terminal moraine, into the sea, by the great glacier which descended Nant-Francon.

Mr Trimmer* reports, on the *authority of some workmen*, that sea-shells have been found on Moel Faban, two miles N. E. of Bethesda. I ascended this and some neighbouring hills, but could find no trace of any deposit likely to include shells. This hill stands isolated, out of the course of the glaciers from the central valleys; it exceeds 1000 feet in height, its surface is jagged, and presents not the smallest appearance of the passage of glaciers; but high up on its flanks (and perhaps on its very summit), there are large, angular, and rounded boulders of foreign rocks.

Along the sea-coast between Bangor and Caernarvon, and on the Caernarvonshire plain, I did not notice any boss-formed hillocks of rock. The whole country is, in most places, concealed by beds of till and stratified gravel, with scattered boulders on the surface; some of these boulders were scored. From the account given by Mr Trimmer† of his remarkable discovery of broken fragments of Buccinum, Venus, Natica, and Turbo, beneath twenty feet of sand and gravel, on Moel

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* Proceedings of the Geological Society, vol. i. p. 332; or Phil. Mag. S. 2, vol. x. p. 143. Mr Trimmer was one of the earliest observers of the scores and other marks on the rocks of North Wales. He also remarked, that "some of the larger blocks amid the gravel have deep scratches on their surface." Mr Trimmer himself found broken sea-shells in the diluvium at Beaumaris.

† Proceedings of the Geological Society, vol. i. p. 332; Phil. Mag. *loc. cit.*

Tryfan (S. E. of Caernarvon), I ascended this hill. Its height is 1192 feet* above the sea; it is strewed with boulders of foreign rock, most of them apparently from the neighbouring mountains; but near the summit I found the rounded chalk-flints,† and small pieces of white granite, alluded to by Dr Buckland. Its form is conical, and it stands isolated; wherever the bare rock protrudes its surface is jagged, and shews no signs of being in any part worn into bosses. The contrast between the superficial part of the bare rock on this hill and on Moel Faban, with that of the rocks within the great central valleys of Caernarvonshire, is very remarkable; it is a contrast of precisely the same kind as may be observed in these same valleys by ascending on either side above the reach of the ancient glaciers. A little way down the hill, a bed two or three feet in thickness, of broken fragments of slate mixed with a few imperfectly rounded pebbles and boulders of many kinds of rock, is seen in several places to rest on the slate, the upper surface of which, to the depth of several feet, has been disintegrated, shattered, and contorted in a very curious manner. The laminated fragments, however, sometimes partially retain their original position.

I did not succeed in finding any fragments of shells, but near the summit of the hill, on the eastern or inland side, I found beds at least twenty feet in thickness of irregularly stratified gravel and boulders, with distinct and quite defined layers of coarse yellow sand, and others of a fine argillaceous nature and reddish colour. These beds closely resemble those of Shropshire and Staffordshire, in which are found (as I have myself observed in very many places) fragments of sea-shells, and which every one, I believe, since the publication of Mr Murchison's chapters on the drift of these counties, admits are of submarine origin. It may therefore be concluded, that the layers of coarse and argillaceous sand, and of gravel, with far transported pebbles and boulders, do not owe their origin to an inundation, but were deposited when the summit of

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* Murchison's Silurian System, p. 528.

† I may mention, that at Little Madely, in Staffordshire, I have found halk-flints in the gravel beds, associated with existing species of sea-hells.

Moel Tryfan stood submerged beneath the surface of the sea. As there are no marks of the passage of glaciers over this mountain (which, indeed, from its position, could hardly have happened), we must suppose that the boulders were transported on floating ice; and this accords with the remote origin of some of the pebbles, and with the presence of the sea-shells. Within the central valleys of Snowdonia, the boulders appear to belong entirely to the rocks of the country. May we not conjecture that the icebergs, grating over the surface, and being lifted up and down by the tides, shattered and pounded the soft slate-rocks, in the same manner as they appear to have contorted the sedimentary beds of the east coast of England (as shewn by Mr Lyell*) and of Terra del Fuego? Although I was unable to find any beds on Moel Faban likely to preserve sea-shells, yet, considering the absence of the marks of the passage of glaciers over it, I cannot doubt that the boulders on its surface were transported on floating ice.

The drifting to and fro, and grounding of numerous icebergs during long periods near successive uprising coast-lines, the bottom being thus often stirred up and fragments of rock dropped on it, will account for the sloping plain of unstratified till, occasionally associated with beds of sand and gravel, which fringes to the west and north the great Caernarvonshire mountains.

In a paper read before the Geological Society,† I have remarked that blocks of rock are transported by floating ice under different conditions; 1*st*, by the freezing of the sea, in countries where the climate does not favour the descent of glaciers; 2*d*, by the formation of icebergs by the descent of glaciers into the sea, from mountains not very lofty, in latitudes (for instance in that of Geneva, or of the mouth of the Loire, in the northern hemisphere) where the surface of the sea never freezes; and, 3*d*, by these two agencies united

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* "On the Boulder-Formation of Eastern Norfolk," Phil. Mag. § 3, vol. xvi. May 1840, p. 351.

† May 5. 1841, "On the Distribution of the Erratic Boulders, and on the contemporaneous unstratified deposits of South America" (Phil. Mag. S. 3, vol. xix. p. 536).

I have further remarked, that the condition and kind of the stones transported would generally be influenced by the manner of production of the floating ice. In accordance with these views, I may remark, that it does not seem probable, from the low level of the chalk-formation in Great Britain, that rounded chalk-flints could often have fallen on the surface of the glaciers, even in the coldest times. I infer, therefore, that such pebbles were probably inclosed by the freezing of the water, on the ancient sea-coasts. We have, however, the clearest proofs of the existence of glaciers in this country; and it appears that when the land stood at a lower level, some of the glaciers, as in Nant-Francon, reached the sea, where icebergs charged with fragments would occasionally be formed. By this means we may suppose that the great *angular* blocks of Welch rocks, scattered over the central counties of England, were transported.* I looked carefully in the vallies near Capel-Curig and in Nant-Francon for beds of pebbles, or other marks of marine erosion, but could not discover any; when, however, Moel Tryfan and Faban stood beneath the level of the sea, inland creeks of salt water must have stretched far up or quite through these valleys, and where they were deep, the glaciers (as at present in Spitzbergen), would have extended, floating on the surface of the water, ready to become detached in large portions. From the presence of boss-formed rocks low down in the valley of Nant-Francon, and on the shores of the lakes of Llanberis (310 feet above the sea), it is evident that glaciers filled the valleys after the land had risen to nearly its present height; and these glaciers must have swept the valleys clean of all the rubbish left by the sea. As far as my very limited observations serve, I suspect that boss or dome-formed rocks will serve as one of the best criterions between the effects produced by the passage of glaciers and of icebergs.† Dr Buckland has described, in detail, the marks of the passage of glaciers along nearly the whole course of the great central Welch valleys; I observed that these marks were evident at the height of some hundred feet on the mountain-sides above the water-sheds, where the streams flowing into the sea at Conway, Bangor, Caernarvon, and Tremadoc, divide: hence, it appears, that a person starting from any one of these four places (or from some way up the valley where the glacier ended), might formerly, without getting off the ice, have come out at either of the other three places, or low down in the valleys in which they stand. The mountains at this period must have formed islands, separated from each other by rivers of ice, and surrounded by the sea. The thickness of the ice in several of the valleys has been great. In the vale of Llanberis I ascended a very steep mountain, E.NE. of the upper end of the upper lake, which slightly pro-

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* On the summit of AshleyHeath in Staffordshire, there is an angular block of syenitic greenstone, four feet and a half by four feet square, and two feet in thickness. This point is 803 feet above the level of the sea. From this fact, together with those relating to Moel Tryfan and Faban, we must, I think, conclude that the whole of this part of England was, at the period of the floating ice, deeply submerged. From the reasons given in my paper (Phil. Trans., 1839, Phil. Mag. S. 3, vol. xiv. p. 363), I do not doubt, that, at this same period, the central parts of Scotland stood at least 1300 feet beneath the present level, and that its emergence since has been very slow. The boulder on Ashley Heath probably has been exposed to atmospheric disintegration for a longer period than any other in this part of England. I was, therefore, interested in comparing the state of its lower surface, which was buried two feet deep in compact ferruginous sand (containing only quartz-pebbles from the subjacent new red sandstone) with the upper part. I could not, however, perceive the smallest difference in the preservation of the sharp outlines of its sides. I had a hole dug under another large boulder of dark green felspathic slaty rock, lying at a lower level; it was separated by 18 inches of sand (containing two pebbles of granite, and some angular and rounded masses of new red sandstone), from the surface of the new red sandstone. One of the rounded balls of this latter stone had been split into two, and deeply scored, evidently by the stranding of the boulder.

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* Dr Martens on the Glaciers of Spitzbergen, Edin. New Phil. Journ. 1841, vol. xxx. p. 288.

† In the Appendix to my Journal of Researches (1839), I endeavoured to shew that many of the appearances attributed to debacles, and to the movements of glaciers on solid land, would, in all probability, be produced by the action of stranded icebergs. I have stated (p. 619) on the authority of Dr Richardson, that the rocky beds of the rivers in North America which convey ice are smoothed and polished; and that (p. 620) the icebergs on the Arctic shore drive before them every pebble, and leave the submarine ledges of rock absolutely bare.

jects where the valley bends a little. For the lower 1000 feet (estimated, I think, correctly), the marks left by the glacier are very distinct, especially near the upper limit, where there are boulders perched on the bosses of rock, and where the scores on the nearly vertical faces of rock are, I think, more distinct than any others which I saw. These scores are generally slightly inclined, but at various angles, sea-ward, as the surface of the glacier must formerly have been. But on one particular face of rock, inclined at an angle of somewhere about 50°, continuous, well-marked, and nearly parallel lines sloped upwards (in a contrary sense to the surface of the glacier) at an angle of 18° with the horizon. This face of rock did not lie parallel to the sides of the main valley, but formed one side of the sloping end of the mountain, over and round which, the ice appears to have swept with prodigious force, expanding laterally after being closely confined by the shoulder above mentioned. At this point, where the glacier has swept to the westward and has expanded, its surface seems in a short space to have declined much; for on a hill lying about a quarter of a mile NW. of the shoulder, and forming a lower part of the same range (it stands S.SE. of the Victoria Inn, and has a reddish summit), the marks of the passage of the glacier are at a considerable lower level. At the very summit, however, of this hill, several large blocks of rock have been moved from their places, as if the ice had occasionally passed over the summit, but not for periods long enough to have worn it smooth.

I cannot imagine a more instructive and interesting lesson for any one who wishes (as I did) to learn the effects produced by the passage of glaciers, than to ascend a mountain like one of those south of the upper lake of Llanberis, constituted of the same kind of rock and similarly stratified, from top to bottom. The lower portions consist entirely of convex domes or bosses of naked rock generally smoothed, but with their steep faces often deeply scored in nearly horizontal lines, and with their summits occasionally crowned by perched boulders of foreign rock. The upper portions, on the other hand, are less naked, and the jagged ends of the slaty rocks project through the turf in irregular hummocks; no smooth bosses, no scored surfaces, no boulders are to be seen, and this change is effected by an ascent of only a few yards! So great is the contrast, that any one viewing these mountains from a distance, would, in many cases, naturally conclude that their bases and their summits were composed of quite different formations.

## *Descriptions of some New Crustaceous Animals found in the Firth of Forth.* By Henry D. S. Goodsir, Esq., Surgeon, Anstruther. Communicated by the Author. (No. IV.*) With a Plate.

### SECTION I.—ON THE GENUS MUNNA.

While engaged during the end of July last in examining the produce of a day's dredging from the mouth of the Firth of Forth, I observed a crustaceous animal running briskly along the bottom of the vessel, which I at first took to be a small *nymphon.* On a more minute examination, I found that it was an *Isopodous Crustacean* belonging to the *Famille des Asellotes* of Milne Edwards.

I had applied the name of *Thetis* as a generic title to this animal, and it was my intention to publish it under this name. Some days afterwards, however, as I was accidentally looking over a few numbers of the Isis for last year (1841), I was both delighted and disappointed to find in the No. VI. for that year, my genus *Thetis,* fully described and figured by M. Kroyer of Copenhagen under the name of *Munna.* I, of course, immediately assumed his generic title.

The only species of this genus which I have examined is remarkable in so far that it possesses pedunculated eyes, which are at the same time quite immoveable, and also in the last or filiform portion of the superior antennæ being double.

The external plate of the abdominal branchiæ is extremely narrow, and is not composed of two equilateral pieces, as in the other *Assellota*, but consists of one piece only, with an immoveable suture in its mesial line ; it is attached to the body by means of its proximal extremity only.

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* This intimates the fourth of the series of Original Observations on British Crustaceans, as communicated by the Author.

These animals differ also from all the other *Isopodous Crustaceans*, in the great length of their ambulatory legs, which leads the observer at first to suppose them to belong to the *Nymphonidæ* ; its quick and active habits, however, very soon undeceive him.

The habits of this species of *Munna*, like the rest of the *Isopoda*, are interesting ; it is quick and active in its motions, running along the branches of the smaller corallines with great rapidity. I have never observed it swim ; in fact, it is not adapted to this mode of progression. When pursued along the bottom by any larger animals, or with the point of a needle, it runs quickly before it; but often stops suddenly, turns round, and becomes assailant.

The eggs are carried in a large oviferous pouch, which is situated between the thoracic legs, and is composed of four large plates, very like those of the *Caprellæ*. I now subjoin M. Kroyer's definition of this genus.

### MUNNA (*Kroyer.*)

*" Novum Isopodum Genus (inter Asellota, Latr. prope Jaeram).*

"Oculi valde prominentes (fere pedunculati), tota capitis latitudine distantes ; antennæ inferiores longissimæ ; pedes 1 mi paris prehensiles (manu ungueque mobili instructi), reliqua 6 paria ambulatoria, longissima (pleraque corporis longitudinem superantia), biungulata ; 7 mus thoracis annulus minimus parumque conspicuus ; cauda appendicibus omnino destituta ; branchiæ unica tantum tectæ lamina."[*]

In the species which I have examined of this genus, the tail is not, as Kroyer states, entirely destitute of appendages; for although minute, they are easily seen with a small power. They are four in number (Plate VI. fig. 14.), one spine at each of the posterior angles of first abdominal segment, and two smaller styles from either side of the apex of the last joint.

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[*] Kroyer. Isis von Oken, 1841, p. 428.

### MUNNA KROYERI. (*Mihi.*) Plate VI. fig. 2.

With the eyes extremely prominent, and with the whole body very spiny.

*Description.*—The whole animal of an ochrey-brown colour, except the reticulated portion of the eyes, which are black. The head is large, rounded anteriorly, and slightly pointed in the middle. The palpi of the external maxillæ are seen projecting in front of the anterior edge, and they are always in motion. The dorsal surface of the head is quite smooth, and the internal or superior antennæ arise from it within the margin, and a little anterior to the eyes; they are extremely curious, and are composed of a peduncle of three articulations, and of a double multiarticulate setaceous portion, which arises from the last joint of the peduncle; these are very slender, and incline towards one another at their extremities (Plate vi. fig. 1). The whole organ is equal in length to the two first joints of the inferior antennæ and the proximal half of the third joint. The external antennæ are much produced, being considerably longer than the body; the peduncular portion is composed of four joints, the setaceous portion is multiarticulate. Two large blood-vessels are seen running through these organs.

The eyes are large and pedunculated, but quite immoveable; the reticulated portion is small, and is almost altogether confined to the lower surface. The first six thoracic segments of the body are almost all equal; the seventh is obsolete. The first pair of legs are prehensile, and the mechanism is rather curious (Plate VI. fig. 5); the fourth joint is very large and rounded, the inferior angle of its distal extremity is armed with four large and strong teeth, two of which are large, the other two being smaller; the fifth joint is not so large, and is reniform; the sixth joint is long and pointed, bearing at its extremity a strong claw. The following six pair of feet are ambulatory (Plate VI. fig. 10). They are very spiny, and the last joint is armed with two strong claws, which are not placed in the usual way, but with the one above the other; the superior is largest and strongest. The abdominal portion of the body (Plate VI. fig. 14) is composed of two segments. The first, which is largest, is of a square shape, and is armed with two strong spines at its posterior angles. The last segment is almost triangular, the apex being directed posteriorly, two small styles arise from each side of the apex. All the external margins of both of these segments are thickly fringed with minute hairs and spines.

The abdominal branchiæ are almost semicircular, and each of them is armed on its internal edge with a small appendage. Length of body one line; span of legs three lines. (Plate VI. fig. 6).

I dedicate this species to M. Kroyer, the original discoverer of the genus, and a naturalist who has added much to our knowledge of the crustacea of the north of Europe. The *M.*

Boeckii of Kroyer is the only other species of this genus known.[*]

### SECTION II.—ON THE GENUS EVADNE.

The next animal to be described is a *Daphnoid Crustacean;* it is the *Evadne Nordmanii* of M. Loven.

About the end of June and the beginning of July last (1842), innumerable shoals of species belonging to this genus, with immense numbers of *Entomostracous Crustaceans,* ranging under the order *Copepoda* of Milne Edwards, appeared at the mouth of the Firth of Forth. These shoals were most numerous about the sheltered parts of the island of May. And so abundant were they, that by drawing a scum-net once through the water, the animals could be taken out of it in handfuls.

Although I have observed one or two undescribed species amongst those specimens which I have got, I have not been able, from want of time, to examine them minutely. In the meantime, then, I will merely give a short account of the observations I have made on M. Loven's species :—

### GENUS EVADNE (*Loven.*)

The head not detached from the body; the anterior branch of the antennæ composed of three, and the posterior branch of four, articulations.

*Evadne Nordmannii.* Plate VI. fig. 15.

*Description.*—The whole animal almost colourless, except the posterior portion of the eye, which is black; the anterior portion is much larger, and is deeply ribbed longitudinally. The antennæ are composed of two branches, an anterior and posterior; and a number of long spines arise from the extremity of each. Four short articulated legs arise almost immediately below the eye; a strong muscle ascends from the legs, and, passing upwards immediately behind the eye, is attached to the dorsal portion of the shell. Each leg is composed of four articulations, and a number of strong spines arise from these articulations. All that portion of the internal cavity of the shell, immediately behind the muscle, is apparently empty, except during the season of spawning, when this part of the body may be observed filled with ova or young. The posterior part of the body is produced, in the middle, into a strong pointed spine. There is no appearance of an intestinal canal, and its organism is apparently very simple. Length, half a line.

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[*] Kroyer. Isis von Oken, 1841, p. 428.

The habits of this animal are extremely active, and are very similar to those of the *Daphnia.* This species, along with different *Entomostraca,* forms the principal food of the herring during the summer months.

## SECTION III.—ON A NEW GENUS OF PYCNOGONIDÆ.

### GENUS PASITHOE

Forms a new type among the *Pycnogonidæ.* Its generic characters are very distinct. The rostrum is very long, and is armed on either side with a long and powerful palpus, which is composed of eight articulations. No mandibles. Oviferous legs very short, and nine-jointed.

P. VESICULOSA. (*Mihi.*) Plate VI. fig. 17.

*Description.*—The whole animal is of a brown colour. The rostrum is oblong, oval, contracted at either extremity, and considerably swollen in the middle. The palpi are eight-jointed, rather longer than the rostrum, and are armed at their extremities with long hairy spines. The three coxal joints of the ambulatory legs are minute; the second one is considerably dilated at its distal extremity. The femoral and two tibial joints are all equal in length, and each of them is peculiar in shape; about one-third of its proximal extremity is very much contracted, and quite cylindrical; the succeeding portion then swells suddenly out, and is rounded off at its distal extremity. The first or contracted portion of these joints is apt to be mistaken for a separate articulation. The first tarsal joint is minute and rounded; it is only seen from the lower surface. The second tarsal joint is equal in length to the femoral or tibial joints, and is very much bent, and its extremity is armed with the usual number of claws; but the two auxiliaries are very minute. The first thoracic segment of the body is the largest, being as large as the three others combined. The oculiferous tubercle is situated near the anterior edge; it is small, and the eyes are placed around it. The last or abdominal segment is very long and slender, being almost as long as the rostrum; it ends in a fine point. The oviferous legs are very short; the fifth joint is longest. Span of legs half an inch.

This animal approaches more nearly to the genus *Pycnogonum* than any other species of the family yet known; and, at the same time, many of its characters assimilate it to the genus *Phoxichilus;* thus, I should think, forming the connecting link between these two genera.

### *Description of Plate VI.*

Fig. 1. Superior antennæ of Munna Kroyeri.
... 2. Munna Kroyeri.
... 3. Nat. size of do.
... 4. Inferior antennæ of Munna Kroyeri.
... 5. First or prehensile pair of legs.
... 6. Abdominal branchus, *a*, appendage.
... 7, 8, 11, 12, 13, Parts of the mouth.
... 9. Abdominal plate.
... 10. Ambulatory leg.
... 14. Abdominal segments.
... 15. Evadne Nordmannii, *a*, nat. size.
... 16. Anterior part of body of an Evadne.
... 17. Pasithœ Vesiculosa, *a*, nat. size.
... 18. Rostrum, palp, and oviferous legs of do.

## *Extracts from Professor Valentin's Report on the Progress of Embryology in the year* 1840.*

Some interesting discoveries rendered the past year a highly productive one for embryology. Two main problems which engaged the various physiologists here occupy the foreground; namely, the earliest development of the Mammalia, and the metamorphoses of the germinal membrane in its transformation into the embryo. * * * * * So long as the metamorphoses of the germinal vesicle following fecundation could be considered only hypothetically, it was assumed that the Purkinjean [germinal] vesicle either burst and poured out its contents, or became flattened; and now in one of these two ways contributed to the formation of the germinal membrane. Both theories had been put forth before the discovery of the germinal spot. But when the existence of the latter became known, the discoverer said, that probably the *macula germinativa* represented the first foundation of the germinal membrane. This conjecture obtained more probability from the obvious fact, that the number, size, and distribution of the germinal spots alternated according to the different stages. Research, however, first in the mammalia, and then in reptiles and fishes, shewed that, in consequence of fecundation, the interior of the germinal vesicle presents new cells, or that (as was seen in the rabbit), within the germinal vesicle, new cells are really built up upon the foundation of the germinal spots.—(*Introductory Remarks*, p. 13).

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* [illegible] f r *Anatomie und Physiologie.* Jahrgang, 1841.

*First stages in the development of the fecundated ovum, especially that of the Mammalia.*—As was already remarked in the Introduction, the most important publications of the past year concerning embryology, are concentrated in the subjects of this chapter. We will, therefore, before presenting some extracts of the details, first state the most important results. With few exceptions to be mentioned, all the observations have reference to the Mammalia, and indeed to the rabbit.

1. *At the period of the rut certain changes have already taken place in the ovarium, the* [*Graafian*] *follicles, and the structures appertaining thereto.*—Through an increased congestion of the ovary single follicles become more strongly developed. The germinal spot, which gives the impulse to the formation of new cells, probably undergoes changes of this kind. From the observations of Negries, above-mentioned (p. 248), it may be conjectured that in the human female also, the period of menstruation is attended by similar phenomena.

2. *Fecundation itself comes to pass apparently in the following manner. A portion of the semen that has been brought to the surface of the ovarium probably passes into the ovum, and gives the stimulus to the formation of cells within the germinal vesicle.* * * * * * *

3. *The number of ova prepared for fecundation by the rut, does not correspond with the number of the subsequently fecundated ova, but generally exceeds the same.*—This fact, already known, has been confirmed by the latest researches on the rabbit.

4. *It often happens that more ova pass out of the ovary than are fecundated, or at least than become developed.*—Herein accord the observations of BARRY with those of PAPPENHEIM. The former found in the tubes and uterus unfecundated or aborted ova. In like manner, parts of the [Graafian] follicle which usually remain in the ovary, for example, portions of BARRY's *ovisac*, may be found in the oviducts.

5. *Neither the place to which the ova in the tubes and uterus have advanced, nor the size of the same, nor the time that has*

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Protracting table, Mr Buchanan's, 140.

Publications, new, 210.

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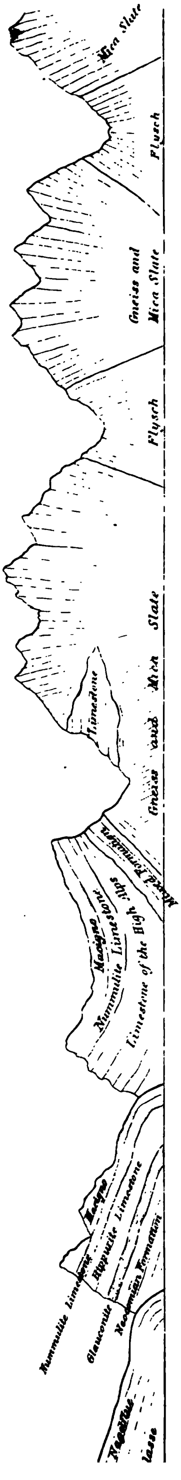
**PLATE I.** Edinr. New Phil. Jour. V. l. 33, p. 144 & 165.

IDEAL SECTION OF THE ALPINE SYSTEM.



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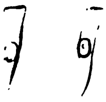

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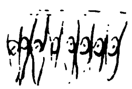

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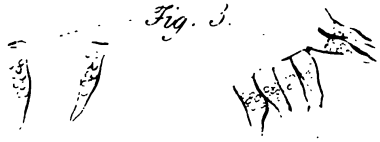

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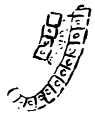

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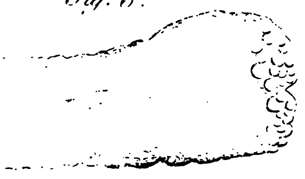

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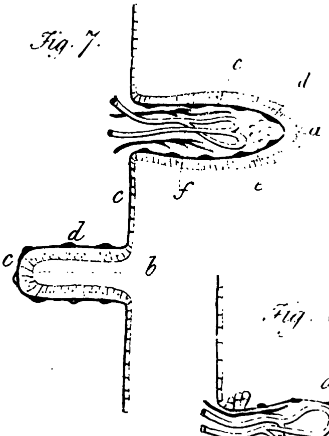

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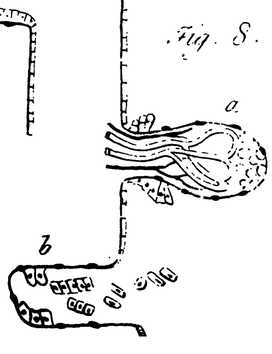

## *Notices of Earthquake-Shocks felt in Great Britain, and especially in Scotland, with inferences suggested by these notices as to the causes of the Shocks.* By David Milne, Esq., F.R.S.E., M.W.S., F.G.S., &c. Communicated by the Author.

(Continued from Vol. XXXII. page 378.

### II. Accounts from more distant parts of the Country.

These have been, for the sake of distinctness, arranged according to the direction of the districts of country from Comrie:—and they will be presented in the following order. Accounts will be given, first from districts *West* of Comrie, next from districts *North* of Comrie, next from districts *East* of Comrie, and, lastly, from districts *South* of Comrie.

### (1) *Accounts from districts West of Comrie.*

At *Dunira*, Sir David Dundas informed the author, that the shock of 23d October, like every other, was felt to come from the NE. The hill of Dunmore, from or near which the shocks appear to originate, is situated to the NE. of Dunira, and is about one mile distant. With regard to this hill, Sir D. Dundas writes, that its ancient name may "induce one to believe, that, in former days also, there had been some queer doings in that district. The ancient name of the hill was Dundownie, or Hill of Evil Spirit; and tradition says that this said Evil Spirit dwelt in the cauldron immediately to the NE. of the hill. The waterfall there is still called the Devil's Cauldron, or Slochk an Down."

At *Ardvoirlich*, about 7 miles west of Comrie, the shock is described by Mr Stewart as having been very severe. "The noise which preceded the shock was as loud and similar in sound to the discharge of a piece of cannon at the distance of a quarter of a mile, and was followed by a rolling noise which lasted from 15″ to 20″. There were two distinct shocks, following each other at the interval of less than a second. I can compare the sound to nothing more nearly, than the report f one of the heavy guns of Edinburgh Castle as heard in the ew Town, when fired on the south side of the Castle. The first report was followed by a long rolling reverberation. In every case, I am inclined to say that the sound proceeded from the atmosphere, and not from under ground. The sound seemed to be high in the air. In every case, it was remarked here to have been heard first in the south-east, and to die away gradually towards the south and west. The shocks were most severely felt in the upper parts of houses. This house is situated upon flat alluvial ground, from 10 to 20 feet above the level of Lochearn, and having a subsoil of coarse, adhesive, ferruginous gravel. The shocks seem to have been more severely felt here, than in the neighbouring farm-houses situated on the slope of the hill. During the whole period of nearly three weeks over which the earthquakes extended, the state of the atmosphere was peculiar. The air was very calm, thick, and heavy, and the temperature high for the season. The hills were almost constantly enveloped in thick mist. Heavy falls of rain were of frequent occurrence,—while the barometer stood unusually high."

At *Glenbuckie*, about 14 miles west of Comrie, Mr Claud Russell, accountant, Edinburgh, who was there at the time, states that the shock was perceived about $10^h\ 15'$ P.M. The noise and concussion both appeared to come from Lochearn. The table in the drawing-room shook, and caused a "dinnaling" noise.

At *Callendar*, about 15 miles SW. of Comrie, the shock was felt about $10^h\ 10'$ P.M. The noise and motion continued for $30''$ or $40''$. "We were most sensible (says a correspondent) of a violent undulating movement of the whole apartment. The tin covers in the kitchen, hanging by rings on long nails on the north-west wall, were set into violent motion, and were shaken off from the wall."

At *Luss* Manse, on west side of Loch-Lomond, a rumbling noise was heard, "accompanied by a tremulous motion which shook the doors, tables, chairs, &c., and caused the bells in the manse to ring. An individual distinctly felt his chair move three times under him."

At *Cameron House* (the residence of the late Admiral Smollet), also on the west side of Loch-Lomond, the shock was perceived about 10 P.M. It produced a sound, in the lower flat of the house, like that caused by carriages. In the attic storey, the noise was much louder, and caused two maid-servants there, to run down stairs to learn the cause of it. At the porter's lodge, the gardener and his wife, who were in bed, were alarmed by the noise, and by the motion of the bed. He described the sound as like distant thunder. The noise travelled from East to West.

Mr Charles Forbes (brother of Professor Forbes), who was at the time *three miles south-west of Glasgow*, wrote to his brother that he felt the shock at 10 o'clock. "I felt the room all tremble, and particularly the timbers below my feet. The lamp shook, and the glasses rattled. I cannot say whether there was noise accompanying it or not, for the shaken house produced a sound. It was as if there had been a very heavy weight knocked five times (I would say) very rapidly on the garret-floor above me, and with sufficient power to shake the whole house. The iron-bars of the windows rattled three or four times." Mr Forbes' house is built on the diluvial clay of the Clyde.

At *Glenmallon*, near Finnart, on Loch-Long, Dumbartonshire, and about 30 miles SW. of Comrie, Sheriff Colquhoun writes that the shock was felt by him at $10^h 10'$ P.M. "I was engaged in the perusal of a book which interested me very much, when suddenly I heard a singular, loud, hollow rumbling noise, resembling nothing I had ever before experienced, followed, almost instantaneously, by a rapid undulatory motion of the floor beneath me, and that again by a violent concussion of the walls of the house, and rattling of the windows. I thought, indeed, at the moment, that the house was about to tumble down about my ears. The whole of these phenomena occurred in the course of two or three seconds of time, and immediately afterwards every thing became perfectly quiet as before. My house fronts nearly SW.; and it appeared to me that the shock approached from behind, *i. e.* from the NE., passing towards the loch, and probably into the opposite lands of Argyleshire. The rumbling sound already noticed, which s perceived immediately previous to the undulation and cussion, was of a very peculiar nature, and caused a mo- tary, confused, uneasy sensation, which I can only com-

PLATE III. Edinr. New Phil. Jour. Vol. 33, p. 174.

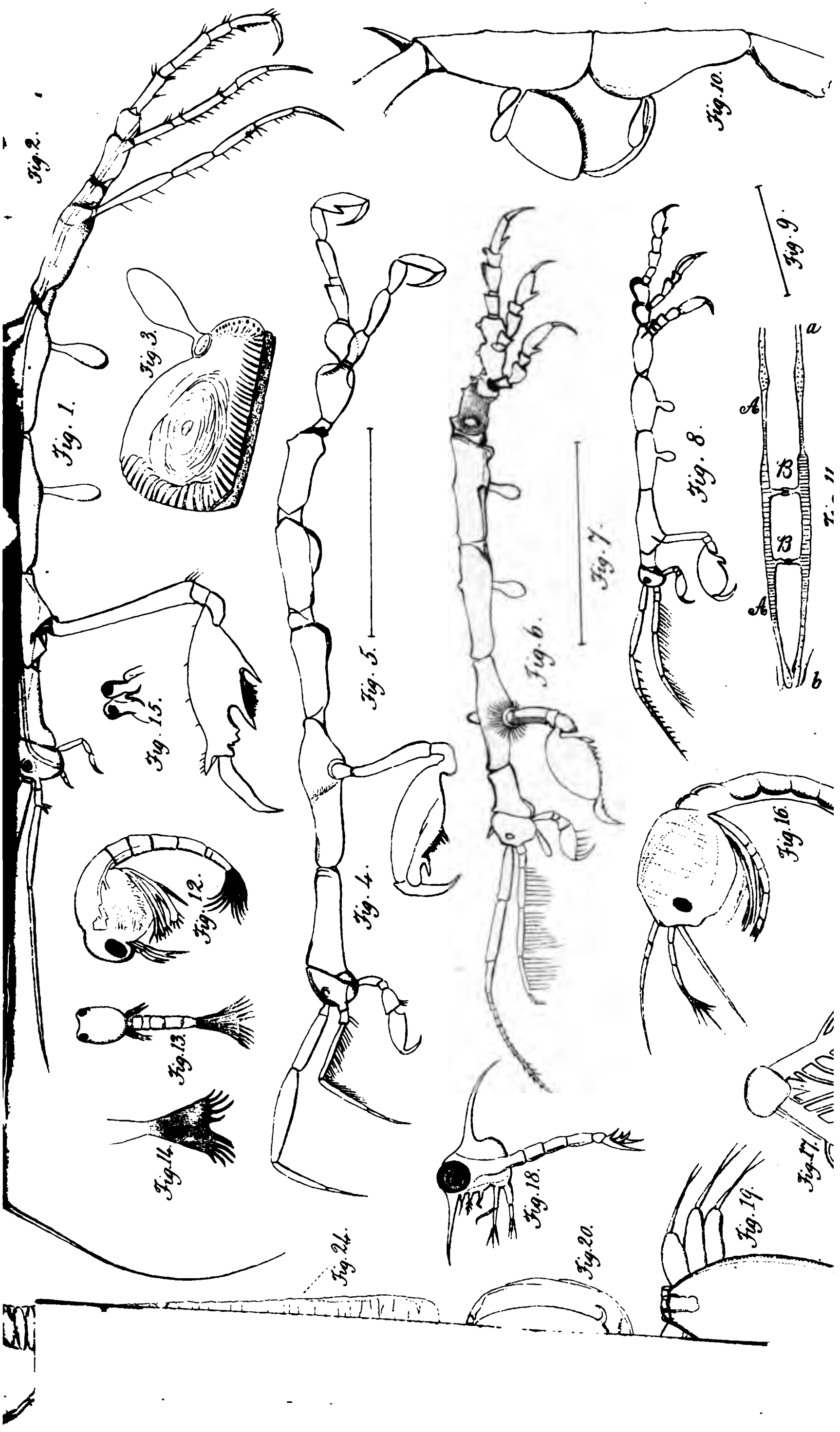
Fig. 1.
Fig. 2.
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Fig. 11
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.
Fig. 19.
Fig. 20.
Fig. 21.
a
A
B
B
A
b

miles from Comrie, and in a direction (by true bearings) N. ½ E. from it, some very remarkable phenomena were observed.

The two gentlemen, whose reports are now to be quoted from, are the Rev. Mr Shepherd, parochial minister there, and the Rev. Mr Rutherfurd; the latter of whom was the person selected by Sir D. Brewster, to take charge of the Meteorological Observatory established at Kingussie.

Mr Rutherfurd states that "the shock took place at 18 minutes past 10 o'clock P.M. One person informed me that another took place about a quarter of an hour after, with a rushing noise like wind, but no tremor; the atmosphere being quite calm, when examined without doors. The noise was almost instantaneously succeeded by a heaving up of the ground, and then, after the interval of a few seconds, succeeded by a vibratory motion. One gentleman described it to me as if a huge giant had attempted to lift the house, and not being able to succeed, took hold of it by the two corners in a passion and shook it from side to side. Another mentioned the bed on which she lay as heaved up, and, after a moment's pause, shaking her from side to side as if to awaken her from a sound sleep. The noise seemed to be in the earth. The shock seems, from concurrent testimony, to have almost followed the track of the river here, nearly in a north-easterly direction. In one house pieces of loose lime fell from the sarking upon the garret floor, and the parlour door was thrown open. In other places the dishes, &c. rattled in the presses. In one the bottles were broken, and the house-bell rung. In one place, a little below the boat-house, where the valley is narrowest, a piece of alluvial level land, almost surrounded by the Spey, has been observed since to have an undulating surface, just as if the waves, during a swell in a rather calm sea, had been arrested in a moment and so remained. The undulations seem to have been moving in a north-easterly direction; they are not very marked, but were not observed before."

"There was a sulphureous smell at the time of the shock, which was most distinctly felt by the Rev. Mr Shepherd, who went out almost immediately after the shock, and called Mrs Shepherd, who also felt it very strong. They repeated their

Fig. 1.

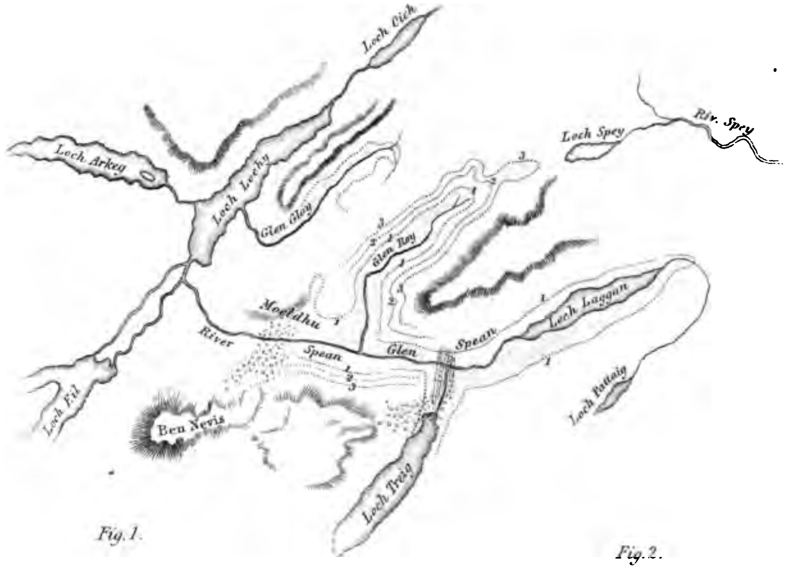


Fig. 1.

…… 1.2 & 3. *The Parallel Roads.*

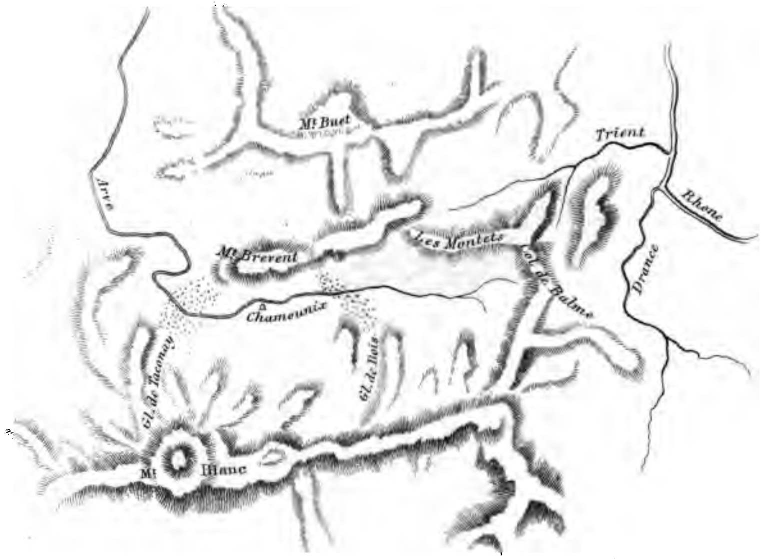
⁘ *Supposed Glaciers.*

||| *Striae of the Polished Rock.*

Fig. 2.

⁘ *Supposed prolongations of the Glaciers de Taconay & de Bois.*

Fig. 2.



undulations could not have been caused by tillage. Moreover, the furrows of the plough were quite distinguishable from the undulations, and ran in a direction nearly at right angles to them. Mr Shepherd immediately made the circumstance known to Mr Rutherfurd, who communicated them to the author. It was then suggested, that inquiry should be made of any persons employed who had been working in the field during that season or the preceding one, so as to ascertain whether the undulations were new to them. This was done; and a precognition of several persons was sent to the author, from which it distinctly appeared that the field had not been ploughed for three or four years previously; and that the furrows of the plough ran in an east and west direction, whilst the undulations ran in a north and south direction (by compass). Farther, these persons, or some of them, stated that they had mowed the grass in 1837, and were sure that no undulations existed then. It had been in 1838 and 1839 pastured with sheep. These undulations were nine or ten in number, and occupied about one-half of the field. They consisted of alternate elevations and depressions on the surface of the ground, parallel to each other. The hollows between the ridges varied in depth from two to eight inches; the deepest being in the middle. The distance between each ridge varied from nineteen to thirty-one feet; the widest being also about the middle of the undulations.

When the author visited the field in 1842 there were still three or four of the undulations perceptible, though it had been ploughed since 1839. He observed that the soil was a stiff, tenacious, and flexible clay, apparently capable of retaining any shape or form impressed on it.

The undulations are at right angles to the general direction of the valley of the Spey, in this part of its course; the river here running nearly due east, by compass.

Whilst the circumstances above noticed, as well as the opinions of the inhabitants connected with the locality, strongly favour the supposition, that the undulations just described were produced by the earthquake of 23d October 1839, it is difficult to understand how such an effect could have been produced. That the vibrations transmitted upwards from the subterranean focus of action, would produce more effect on an alluvial deposit than on almost any other kind of surface, can be readily conceived; and it was clearly shewn in other places nearer Comrie, that an undulatory movement of the earth's surface was produced. But if at Kingussie there was an undulation produced in the haugh-land just referred to, would this undulation consist of more than one, or at the most two, waves? and would they not move progressively forward, leaving the surface as level as formerly? How could the waves or any portion of them have been arrested in their progress, so as to present a series of nine or ten parallel undulations? Is it sufficient explanation, that the surface when raised, say ten inches above its ordinary level, might have sunk back only six or eight inches, and that as the original wave advanced, it might produce successively a similar effect in different parts of the field,—effects which, if produced, would be exhibited in lines coincident with the direction of the moving cause?

It is proper to add, that similar undulations, though less distinct, were perceived in another haugh near Kingussie, on the south side of the river.

At *Inverness*, as the author was informed by Mr George Anderson, " the earthquake was felt about 15′ or 20′ after 10 P. M., and as far west as Fort Augustus. At Inverness, there was no second shock; and in the country around, where such was felt, it was so slight, as only to be perceptible to a very few persons.

" At Inverness the nature of the concussion was that of a mere *tremor*, as if occasioned by the passage of heavy carriages along the street; there was no *undulation*. At Farr, Colonel Mackintosh felt as if the house had received a concussion producing an undulatory motion across it in an easterly direction, which was succeeded by a loud tremulous and rumbling noise, resembling that of wheeled-carriages driven rapidly past the house, in a direction from S.W. to N.E., and lasting about 30 seconds. The writer does not think the duration generally was more than from 20 to 30 seconds.

" The pulsation shook houses and furniture in them, but so slightly, that it was not perceived by many; and in several instances the noise was only thought to be caused by servants

stone). It was also strongly felt at Elgin (town stands on cornstone (limestone of old red), and near it, I am told, two China bowels containing flowers, which stood near each other, were broken. The following extract from a letter from Dr Geddes of Blackhills, 4½ miles east of Elgin, will give you some particulars.

At *Amulree*, about 12 miles N.N.E. of Comrie, the concussion caused a small part of an old road to sink, rendering it impassable. In the neighbourhood of the same place, several fissures were formed, running generally N.N.E. and S.S.W. One of these is described by the Rev. Mr Lamont, as being about 200 yards long, and another about 50 yards long. These two fissures were about 50 yards apart, and united towards the south in a curve. In some places, one of them was several feet in width. The ground contained within the limits of these two fissures, appeared to have moved towards the north:—for a dyke built across this elliptic tract, in an east and west direction, was, where intersected by the fissure, carried a few miles towards the north, and a portion of the dyke was thrown over in the same direction.

### (4.) *Accounts from Districts North-East of Comrie.*

At *Fraserburgh*, on the sea-coast of Aberdeenshire (about 140 miles from Comrie), Mr Jameson writes that his wife was sitting up expecting him to return home, when "she thought she heard the sound of a carriage approaching in the direction she expected it, viz. S. W., and consequently went to the door, but neither saw nor heard anything except a trifling sound like that of a gust of wind dying away; felt no concussion. A gentleman, at Knousie, six miles south from this place, was in bed, and felt a sensation as if something was pulling the bed-clothes off him, and the same thing repeated after an interval of three or four seconds, when he was seized with a kind of fear, thinking it was something extraordinary.

"At *Strichen*, 8 miles S W. from this, two young ladies had retired to their rooms for the night, when, soon after, one ran into the apartment of the other in a fright, saying, what can be the matter, for things are moving and the crockeryware are clattering in the cupboard? No other person in or about Fraserburgh appears to have felt it."

At *Banchory*, situated to the west of Aberdeen, and about 80 miles from Comrie, Mr Innes of Raemoir writes,—"At the time of the earthquake, my daughter was an invalid, and the family had gone to rest. We sleep on the first floor,—our daughter in an adjoining bedroom towards the east. The wind was strong and boisterous. From a quarter to half-past 10 I had fallen into a slumber, when I was roused by what appeared a loud gust of wind, and I felt as if in the bedroom overhead a person had sprung with naked feet on the floor, and run or stamped across. While collecting my thoughts as to what could have caused such an occurrence, and recollecting that there was no person in the rooms above, my daughter rung her bell and opened her door. Her account is, that lying quite awake, the wind for the moment had lulled, she heard as if suddenly a heavy carriage had driven round the house, coming from the west and going off by the east, and her bed shook, and the wardrobe and all the articles in the room rattled. She immediately started into a sitting posture, and, while thinking if any carriage could have arrived, and listening attentively to any sounds, she was attracted by what seemed a peal of thunder, *immediately followed* by a noise overhead as if of a person jumping and stamping as I have described; a dull noise as produced by a heavy naked foot, and *simultaneously* the bed seemed to *heave*, and the wardrobe and other articles rattled as before. She thinks that one or two minutes must have intervened between the two shocks, from the impression left of the thoughts which passed through her mind by the first before the second took place. Being alarmed, she started up and rung her bell. Of the servants, who sleep in the lower part of the house, only one maid appeared to be attracted by it. On the bell being rung, she called to her fellow-servant to get up, as she had heard a carriage drive up. We came to the immediate conclusion that it was an earthquake, and it very much resembled the sensation which accompanied that which occurred, I think, in August 1816, and which I felt in this neighbourhood. All the ground round here is on granite. The hill behind is composed entirely of a mass of red granite, and the foundations of the house cannot be many feet, on a bed of clay raised above the granite. My daughter says that she cannot assign any particular duration to the noise and tremor, but she thinks it could not have exceeded a few seconds, and the tremor on the second occasion seemed to follow within a second or two what she speaks of as like a peal of thunder."

Dr Adams of Banchory states, that in his neighbourhood the shock was accompanied with a sound,—that the shock or concussion lasted for 2″ or 3″, and that the sound continued somewhat longer. " The inmates of Banchory Lodge felt as if the floor was raised at one end and depressed at the other. All agree, that the concussion and noise came from the westward and passed off by the east. One person who felt the shock distinctly while lying in bed, describes it to me as a tremor or vibration : but all the others felt or fancied that they were rocked or lifted up. There seems no doubt as to the fact, that the shock was most felt in the upper parts of houses.

" I have not been able to arrive at any certain conclusion whether the shock was felt more in houses founded on rock than on those built upon sand. It was felt equally strong at Finzean, which is built upon rock, and at Mill of Clinter in the same locality, which is founded upon sand : at Raemor, which is built upon rock, and at Banchory Lodge, which is founded on sand. The general structure of the rocks on Deeside is granite. The limestone-districts are so limited in extent that it is impossible to arrive at any certain conclusion whether the shock was felt with more or less severity in them than in other places. It was distinctly felt in the limestone-district of Tilwhilly in this parish."

At Finzean, a little to the west of Banchory, Mrs Farquharson, at 10$^h$ 30′, felt distinctly " one shock or concussion which lasted about 2″, followed by a low rumbling noise which continued about 4″. The noise was in the earth. I was sitting by the fire, reading, when I felt the shock. My chair seemed to sway as from the motion of a ship on a wave of the sea. I called to Mr Farquharson, who had just gone to bed ' what is that ?' He answered, ' I do not know,—but the bed is shaking under me;' and immediately added, 'It is an earthquake.' The south-west part of the house seemed to be first struck, and I felt myself swayed from the south to the north,—in which direction also the shock and noise seemed to travel."

At the Manse of *Aboyne*, which is built on sand, the shock was, as the Rev. Mr Milne Miller reports, "preceded by a rushing noise not unlike that produced by the rolling of a violent wave on a very pebbly shore: noise heard several seconds before shock was felt. And it is worthy of remark, that I thought the noise came rushing from the west till the instant the shock was felt, and it then gradually died away toward the east.

"At the instant of the shock a noise (in addition to the rushing noise) heard in room above the one I was sitting in, like that produced by a person walking heavily on the floor, or perhaps dragging a heavy body along floor. I remarked the same in 1816.

"Manse shook or rather rocked a little; roof creaked as if a rafter had been giving way. Several tin covers in my kitchen continued to vibrate or rattle against wooden partition on which they were hanging for some seconds after the shock. West gable of manse appeared to be first and most affected. I infer (in addition to my own feelings at the time) that the shock was from the west in regard that the tin covers rattled on partition, which is in a *meridian* position; for had partition extended in same direction as course of shock, anything hanging on it would not so likely have rattled against it. I felt as if chair had been suddenly lifted up under me, and then rocked a little from west to east. I should say the late earthquake was of longer duration, but of duller sound and motion, than the one I felt at Fort George in 1816."

A farmer on Deeside, about 33 miles west of Aberdeen, when lying in bed heard the slates on the roof of his house rattle.

At *Aberdeen*, as Professor Cruickshanks writes, the shock and noise were slightly felt. No ship or boat in the harbour was moved, nor were the instruments in the observatory affected. There was no displacement of furniture, but much rattling of glasses and stoneware.

At *Blair Gowrie*, and in the glens of the Shee and the Airdle, situated in the mica and clay slate formations, about forty miles N.E. of Comrie, the effects of the shock are thus described by the same correspondent, whose account of the shock felt on the 12th October has already been given. He says,—"This shock was much more severe, and was very sensibly felt in this town and neighbourhood, and very strongly in Glenshee and Strathardle. In this town and its vicinity, the shock was preceded by a sound similar to that already described, which was immediately followed by a tremor or vibration of the earth, making the furniture rattle and tremble; and, in one or two instances, forcing open doors that were partly shut. According to some, the floors of the houses were felt distinctly to *heave* like a wave, and afterwards to *tremble* violently for some seconds. In my own case, the feeling was as if a strong gust of wind had suddenly swept over the house with a hollow sound, making the doors and windows rattle.

"In the valleys of Glenshee and Strathardle, as already mentioned, the concussion was felt much more violently, more especially in the former, where several persons were awakened from their sleep by the motion, and felt very much alarmed. Some who happened to be standing outside the houses, and close to the walls, felt as if the houses would fall to the ground. The person already mentioned, who felt the former shock in the hill, states that he felt his bed as it were lifted up from the west or north-west, and let down again, like a boat by a wave, and immediately afterwards the plates and other furniture rattled strongly. Another person, who happened to be sitting with his back to a wall, which ran in a direction from north to south, felt it incline over from the west, and rose in great alarm, under the idea that it would immediately fall. At the Spittal of Glenshee, and also at Kirkmichael in Strathardle, the concussion was very strongly felt, and in the same manner. The most general opinion seems to be that the *noise* accompanying the concussion proceeded from *above*. The sound continued for about half a minute, and the tremor nearly a minute. The shock occurred about 20 minutes past 10 P.M. Two other shocks were subsequently felt, one about 11 on the same evening, and the other about 2 o'clock next morning, but they were very slight compared with the one already described.

"For some weeks previous to the occurrence of the earthquake the weather had been wet and rainy to a most unprecedented degree; and during the day, on the evening of which the most severe concussion was felt (23d October) it rained incessantly, and with great violence. There was a pretty strong breeze of wind from the east, and the eastern horizon presented a dull reddish appearance during the whole evening. During the night, also, the river fell rapidly several feet below the level it had reached in the afternoon of the 23d, notwithstanding the continuance of the rain."

*(To be continued.)*

## *On the Succession and Development of Organised Beings at the Surface of the Terrestrial Globe; being a Discourse delivered at the Inauguration of the Academy of Neuchâtel.* By Professor Louis Agassiz.*

Gentlemen,—The important event which has called us together, seems to indicate to me in some measure the nature of the subject which I ought to select for discussion, in addressing you for the first time as Professor of our infant Academy. It is with all public institutions, as with human life, in the course of which certain epochs are more prominently marked than others, and seem to call us to more serious reflections. Nature also has her important epochs, and I think that the appearance, the development, and the disappearance of organised beings at the surface of the terrestrial globe, merit more particularly being considered under this point of view. The scientific results to which my researches on this subject have conducted me, will not, I hope, be without a corresponding parallel as regards the establishment for the higher kind of public instruction which our Prince, in his solicitude for the intellectual development of our country, has instituted among us.

At the present time, one great thought prevails in natural historical studies, and divides those who have reflected on the facts established by observation; this is the investigation of the origin of living beings, and of the connection which has existed between them at all the epochs of change through which the earth has passed. Of what nature is this connection? What opinion are we to form in regard to those constantly recurring discussions on the succession of living beings, on the links which unite them, on the gaps which it is pretended exist among them, on their similarity, and on their appearance at different periods?

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* Communicated by Professor Agassiz.

There was a time when the earth was uninhabited; and there is therefore in its history an epoch when life manifested itself for the first time on its surface, by producing a diversity of animal and vegetable forms, quite different from those which we see existing and reproducing themselves under our eyes. Further, the different types of animals and of vegetables have undergone notable transformations in the various phases of the history of the earth, transformations which separate us from that first appearance of living beings to such an extent, that at each of the great geological epochs the animals and plants have been very different from what they were at other times. These results have been obtained by science, through the exertions of geologists; and if the whole of antiquity is not to be doubted, it is plain, that the ancient philosophers, in order to construct the world, rather interrogated the secrets of human nature, than the external nature which surrounded them. And yet how many cosmogonies exist! All nations have their own, and all have converted them into religious dogmas. Without pausing to examine doctrines as contradictory as they are superficial, let us see what we are taught by facts laboriously collected during the last few centuries.

Wherever the hand of man has opened up the bowels of the earth, wherever changes at the surface have exposed the deeply-seated layers of its crust, wherever time has fractured its solid masses, the observing eye discovers traces of beings which no longer exist; here there are the debris of mammifera, or reptiles, whose forms are as colossal as they are strange; there the whole rocky mass seems composed of the debris of microscopic animalcules, which escape even the most practised eye. Far from the coasts of the sea, beds of oysters, and the holes bored by pholades on the flanks of mountains, seem to indicate ancient shores. In other localities, numerous remains of fishes, and immense banks of corals lying in their natural position, compel us to admit that our solid lands have formerly been submerged, and that the beds composing our loftiest mountains occupied the bottom of the sea, before they elevated their daring summits to the sky.

At first sight nothing but confusion appears in these masses of debris; and, like Cuvier, we are tempted to compare them to an immense overthrown cemetery, so many members of various animals being mixed together pell-mell. But just as the antiquary has been able, by dint of assiduous study, to recognize, in the ruined monuments of ancient nations, evident traces of several distinct civilizations, of which written history makes no mention; so, in like manner, it was reserved for modern science to lay hold of the impress of the different epochs which have succeeded each other at the surface of the globe. This impress once recognized, investigation necessarily led to much more precise results, inasmuch as the laws of nature are not subjected to those veerings which, in the history of nations, betray every moment human inconstancy. It is thus that a comparative examination has taught geologists to recognize, in the midst of the greatest derangements, the order of succession of all the leaves of which the crust of the earth is composed; and if, in this immense book, there are still some obscure passages among these leaves, inquirers have not been the less successful in ascertaining the exact connection which exists between the different ages of the earth. With such results before us, we are no longer permitted to adopt an opinion on the history of creation, which does not take these data into account.

Before discussing the connection of the phenomena to which I have just alluded, and before searching for its import, let me be permitted to analyze them briefly, restricting myself to the facts relative to the animal kingdom, with which I am more specially occupied. When we study the remains of organized beings which we find buried in the beds composing the earth's crust, we are soon struck at finding that the order in which they succeed one another, from above downwards, and from below upwards, does not at all harmonize with the systems taught, and which formerly represented the whole of these organized beings as forming a graduated series, rising without interruption from beings the most imperfect to man, who now reigns supreme on the earth; nor yet with that other opinion which, denying all succession, sees nothing in the whole of creation but a variegated assemblage of diverse forms, which is to be traced to one and the same epoch, and has no other connecting bond but that of a common existence. Facts equally contradict these two systems, to which all the others may be referred, and of which they are merely varied commentaries.

The most prominent result to which paleontological studies have conducted us, consists in the demonstration of a series of epochs independent of one another, in limits more or less extended, during which living beings have been different. (By an independent epoch I mean a lapse of time during which organized beings presented the same characters, increasing and multiplying by means of generation, and presenting a spectacle analogous to that which we now see every day at the surface of the globe, where numerous very various species live mixed together, and propagate within determinate limits, without undergoing any notable alteration.) These different epochs ought to be regarded as independent of one another, because the differences presented by the debris of organized beings which characterize them do not correspond, in their nature and their intensity, with the modifications which beings now living undergo, in consequence of the influence of time, of climate, and of domesticity. Let us select as an example, an epoch when no reptiles yet existed. Is there any one familiar with the laws of physiology, who will affirm that the first reptile which lived on the earth descended by means of generation, or in any other manner, from any one of the fishes which existed anteriorly? And, continuing the same reasoning with regard to the mammifera and to birds, is it possible to regard them as descending from reptiles? Or such and such a family of carnivorous mammifera, of a more recent period, as descending from some more ancient family of herbivora? Such questions, at the present day, carry with them their own answers; and the objections drawn from the differences observed between the different races of domestic animals, cannot in any way weaken the general principle of the fixity of species. For, to place in the same line, phenomena so different as that of the succession of different species, genera, families, and classes, and the partial and inconstant modifications to which, under the influence of man, certain animals which he has attached to himself, and certain cultivated plants, have been subjected, is at once to declare incompetency to discuss questions of this nature.

But because the organized beings of these different ages of nature have not a genetic bond of connection of the nature of a successive sexual procreation, we must not hence conclude that they are not members of one same plan, and that they are not linked together by bonds of a more elevated description, as we shall afterwards find to be the case.

The only real difficulty on this point which remains to be solved, is the rigorous determination of the limits of all these great epochs; for in proportion as the investigation of fossils acquires more precision, the number of these distinct epochs seems to increase. It is already ascertained that the oldest formations, as far as, and including the coal deposits, are characterised by a particular order of things. In the more recent formations from the *grès bigarrés* up to the chalk, a second great epoch has been recognised, differing as much from the first as from the tertiary epoch that succeeded it, the latter terminating before the present creation, to which belong man and his contemporaries. These four great epochs, that may be called the *ages of nature*, are subdivided into distinct periods, which are equally characterised by several peculiar features.

If it were allowed me to enter into some more circumstantial details, I would add, that those who believe that during the first epoch there existed only animals of an inferior organization, are strangely deceived. Far from that, from the earliest period, the four types of the animal kingdom have been represented at the surface of the globe: the Radiata, Mollusca, Articulata, and Vertebrata appeared simultaneously as the first inhabitants of the earth; and at each of the following epochs, new types of these same great groups reappeared in a different assemblage. Nevertheless, notwithstanding this unity in the general plan, the greatest diversity prevails in its development: the vertebrata of the first epoch are fishes, and fishes only, associated with Articulata, with Mollusca, and with Radiata, of species differing from those which presented themselves afterwards. Thus we may regard this first age as characterised by the *reign of fishes.*

During the secondary epoch, it was no longer the inhabitants of water which alone peopled the submerged surface of the earth; the class of reptiles appeared with a cortége of Articulata, of Mollusca, and of Radiata unknown in the preceding age, and the fishes of this second great epoch assumed a character which those of the first did not at all possess. Strange monsters, of fantastic form, and of gigantic size, bringing to our minds the fabulous dragons and harpies, then peopled the sea and the earth; and although some beings of a superior organization had already begun to shew themselves, the epoch of the secondary formations may be characterized as the *reign of reptiles.*

At the same time a vegetation, of which none of the various floras of our epoch can give us a just idea, was developed during that remote period.

If we pass to the examination of the tertiary formations, the scene at once changes. Numerous mammifera, heavy pachydermata, ruminants of colossal forms, singular cetacea, and birds, besides reptiles and fishes more and more resembling those which live at the present day, without, however, being identical with them, form the varied fauna of this epoch. A rich vegetation was distributed over a more diversified surface, but was still shared unequally by the solid land and the ocean. The climate was more varied than formerly. This was the *reign of the mammifera.*

Corresponding with these changes in the nature of organised beings, others took place in the aspect of the surface of our globe. Every thing leads us to believe that after the consolidation of a first crust, when the waters had begun to accumulate at its surface, our earth did not at all exhibit in its relief the inequalities which we now see. It is, in fact, proved, that the different chains of mountains were elevated successively; so that at different epochs, the boundaries of the solid land and the ocean must have presented different combinations. It is also ascertained that in the most ancient periods, the water occupied a much larger extent of the surface than at present; inasmuch as the most ancient beds in which fossils are found, contain traces of aquatic animals and plants only; whereas we afterwards meet with immense accumulations of debris of plants indicating a terrestrial flora. These are the plants which are converted into coal. The appearance of terrestrial animals is still more recent; for it does not seem to reach a more remote period than the earliest portion of the secondary epoch; and it is only much later, towards the termination of the cretaceous epoch, and during the tertiary epoch, that the solid land appears to have acquired sufficient extent, and to have presented differences of level sufficiently great, to admit of the formation of fresh-water lakes.

A very remarkable, and perhaps the most surprising fact, is, that the appearance of the chains of mountains, and the inequalities of the surface resulting from it, seem to have coincided generally with the epochs of the renewal of organised beings. Hence, what can be more natural than to suppose that the great diversity of aspect presented by the earth, in consequence of all these changes, was calculated to present to man the most varied conditions of development? This opinion appears, in some measure, confirmed by the history of the human race, which exhibits to us the development of the most perfect civilization on continents of the greatest diversity of surface, whereas the least intelligent races generally inhabit the monotonous and uniform regions.

Up to the termination of the tertiary epoch, the law of destruction was paramount. Man did not then exist. Before his appearance, the earth had once more to undergo dreadful convulsions, which produced the elevation of the greatest chains of mountains. It was only after this last revolution that he was called into existence, along with all the beings which now live with him on the earth; and thenceforward we find unfolding itself that long history of our race, imposing the laws of its intelligence on the whole of nature. For the first time a sort of privileged being ruled over nature, and advanced to greater perfection by divesting himself of the animal character which connected him with other creatures, in order to emancipate those intellectual and moral faculties, which recall in him the image of his Creator.

It evidently results from the whole of the facts, and from their connection, that, notwithstanding the apparent independence of these great epochs, notwithstanding the absence of genealogical connection in the different species which characterize each of them, the order of their succession presents a plan in which they are closely linked together. We see, in fact, that to the reign of fishes succeeded the reign of reptiles; to the latter the reign of the mammifera; and in the last place only, the reign of man. But these three classes of animals exhibit in their succession a progressive gradation of organization, as we shall presently find. Abstracting all geological ideas, and apart from all connection with the epoch of their appearance on the earth, the class of fishes has always been regarded by naturalists as inferior to the three other classes of the vertebrata. The form of their body, the absence of distinction between the head and the other parts of the body, the imperfection of their locomotive members, which are only balancing organs, destined to maintain their equilibrium, while the entire mass of the body contributes to make them progress; the existence of branchiæ in place of lungs, as a respiratory organ; the simple circulation of their blood; the remote relations of the sexes; the small degree of intensity of their sensations; the imperfection of the organs of sense; the smallness of the brain; and their obtuse intellectual faculties—everything in their organization has assigned them a rank which no one has proposed to elevate. But however inferior their organization, and though they occupy the lowest rank in the class of the vertebrata, they are so much the more interesting to the observing naturalist; for they are the point of departure of a graduated series which commences with them and by them, to terminate in man himself.

I should transgress the limits I have traced out for myself, were I to undertake to prove that the class of reptiles is intermediate between that of fishes and those of birds and the mammifera, and that these last approach very nearly to man in their organization; so that, regarded as a whole, these four classes seem to be the successive degrees in the manifestation of the type of the vertebrata.

Invertebrate animals do not appear to be subjected to the same laws of developement as vertebrate animals. For, what superiority can we assign to the Vermes, which form a part of the Articulata, over the Cephalopods, which belong to the Mollusca? and on what grounds can we place the Acephala above the Echinodermata, which are nevertheless true Radiata? The truth is, that the existence of the Invertebrata cannot be referred to the same principle which is manifested in the development of the Vertebrata, which latter are undoubtedly linked with the existence of man. Ascending to the epoch of the first appearance of fishes, we find that the Radiata, the Mollusca, and the Articulata, have pursued a series of metamorphoses, which has not at all elevated them to higher types. The corals of the most ancient formations are analogous to those of our seas. The Echinodermata go back equally far; and if we notice the important modifications in their relations with the surface, and in the distribution of their families in the different geological epochs, we find no indication of their genetic connection with the other classes. The same is the case in the three classes of Mollusca; the Acephala of the early periods are, it is true, less free, their symmetry is not marked in so decided a manner on the sides of the longitudinal axis of the animal, the anterior and posterior regions of the body are not so clearly defined, the variety of species, genera, and families, is less considerable than in more recent epochs; but, notwithstanding all that, they advance in a parallel line with the Gasteropods and the Cephalopods, which have at no period been subjected to a greater amount of modifications. With regard to the Articulata we may make the same observation, notwithstanding the imperfection of our information respecting the fossil species of that division. The Crustacea, which are placed at the head, have not by any means been preceded by the insects and the Vermes; any more than the Cephalopods have been by the Gasteropods and the Acephala, and the Echinodermata by the Medusæ and Polypi.

Nothing is more worthy of our attention than the simultaneous appearance of the nine classes of invertebrate animals, and we can only understand it by regarding these animals as manifestations of particular tendencies of life, the principle of which goes back as far as that which is displayed in the appearance of vertebrate animals. But how great is the difference in the case of the latter! Of these there are only four classes, and these classes made their appearance successively at the periods and in the order of their organic gradation. There is here a real progress in the manifestation of the organic characters which successively appeared, according as at each epoch a new and higher class became detached from the first trunk, while creation was approaching its termination.

In regarding the whole animal kingdom in this point of view, we cannot fail to recognise a premeditated plan, connected together in all its parts. The idea of a superior intelligence, independent of creation, and which from the earliest time fixed its phases, at once presents itself. It would be impossible reasonably to attribute such a linking together in the epochs of creation to a power unconscious in itself, acting without rule, or according to immutable laws. A more powerful intervention than the organic forces of nature, reveals itself to our intelligence in this succession of living beings endowed with a temporary stability, and giving place, after having existed without modifications during a given time, to other beings whose duration was to be equally transient. To whatever influences recourse may be had as regards the finished world, we cannot conceive of the spontaneous formation of living beings by the sole action, or by the combination, of physical forces. But here we must at the outset make a distinction between the establishment of the order of things which has regulated the whole of nature from the commencement, and has been maintained throughout all time, and the particular acts of creative will, which have only operated for the establishment of particular portions forming part of the general plan and in some measure only its consequence. The time is therefore arrived when science likewise can recognise in nature God the Creator, the Author of all things, as he was given to man to be recognised in his own heart when he reflected upon himself.

But here the task which the naturalist ought to impose on himself by no means terminates. If it is an obligation on science to proclaim the intervention of a divine power in the development of the whole of nature, and if it is to that power alone that we must ascribe all things, it is not the less incumbent on science to ascertain what is the influence which physical forces, left to themselves, exercise in all natural phenomena, and what is the part of direct action which we must attribute to the Supreme Being in the revolutions to which nature has been subjected. For a long period moralists have been endeavouring to trace the limits of human responsibility, and to fix the degree of liberty which is devolved on man by his nature. It is now time for naturalists to occupy themselves likewise, in their domain, in inquiring within what limits we can recognise the traces of a divine interposition, and within what limits the phenomena takes place in consequence of a state of things immutably established from the beginning of creation.

Let me endeavour to give more precision to what I mean. If the course of the stars does not present to us any variation, if the order of the seasons is immutable, if the reproduction of species always takes place in the same manner, it is evident that the cause of these phenomena is regulated in an unvarying manner, and follows natural laws, independent of the creative will which established them. But if, on the other hand, we see in the beds of the crust of the globe a succession of organised beings such as no longer makes its appearance, and such as man has never seen appearing, such, in fine, as our intelligence cannot conceive appearing spontaneously under the simple influence of the forces of nature, we must attribute its creation to a Supreme Intelligence, which has regulated from the beginning of time the order of the world.

Let it not be said that it is not given to man to sound these depths: the knowledge he has acquired of so many hidden mysteries in past ages promises more and more extended revelations. It is an error to which the mind, from a natural inclination to indolence, allows itself too easily to incline, to believe impossible what would take some trouble to investigate. We generally rather prefer imposing limits to our faculties, than increasing their range by their exercise; and the history of the sciences is present to tell us, that there are few of the great truths now recognised, which have not been treated as chimerical and blasphemous, before they were demonstrated.

I now pause, in order that I may not digress from my subject, and I terminate this discourse by briefly recalling the points on which I have insisted. The earth has its history, a history as rich in great events as it is long to narrate, and of which geology is now successfully collecting all the details. But the facts whose certainty is generally recognised, have also their instruction for us. The history of the earth proclaims its Creator. It tells us that the object and the term of creation is man. He is announced in nature from the first appearance of organised beings; and each important modification in the whole series of these beings is a step towards the definitive term of the development of organic life. It only remains for us to hope for a complete manifestation in our epoch of the intellectual development which is allowed to human nature. May the establishment whose inauguration has this day assembled us, be one day reckoned among those institutions which shall have contributed to this great object!

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## *Account of Observations recently made on the Glacier of the Aar.*

By Professor Agassiz.

M. Agassiz has addressed a letter to the French Academy of Sciences, dated from the glacier of the Aar, 1st August 1842, in which we find the following details:—" For sixty hours, it has not ceased snowing here. The temperature of the air has not risen above + 1 C (33°.8 F.) for two days, and at night it has been at —4° (24°.8 F.). The snow is extremely fine and incoherent; and it falls, for the most part, in the form of a light dust, composed of very small needles very irregularly aggregated, and remaining for a long time suspended in the air before they fall to the earth. This observation invalidates the assertion, so often repeated, that the *névé* in the high regions falls in the granular form which characterizes it. Since I began to visit the Alps," adds M. Agassiz, " I have often seen snow fall in the months of July, August, and September, at heights o

7000 or 8000 feet; and I have frequently examined it shortly after its fall, at heights of 9000 feet and more. But I have never seen it fall in the form of névé; the snow was always in flakes when the temperature was not under 0° (32° F.) at the surface of the glacier, and powdery when the cold was greater.

"Another phenomenon which has struck me in these lofty regions, is the brightness of the nights when the sky is cloudy, and even when it snows or rains. During such weather, we can always distinctly see the hour on our watches at any time of the night; whereas, the obscurity is much greater when the sky is serene. This apparent anomaly recalls the observations of M. Arago on the light of clouds."

M. Agassiz then notices his observations relative to the glacier itself; and first of all as to its progressive movement. It would appear that the movement is much greater at the centre than at the edges; at least, since last year, the centre has advanced 269 feet, the south edge 160, and the north edge 125 feet only.* The ablation of the surface, resulting from the melting and the evaporation, has also been more considerable at the centre than at the edges, contrary to what theory would lead us to suppose. From the beginning of September last year to the 20th July this year, the ablation in the centre has been 6 feet 5 inches, and that at the edge 4 feet 4 inches, without, however, the absolute level of the surface being changed in an appreciable manner. M. Agassiz has likewise noticed, that *crevasses* are more frequent and wider at the edges, especially at places where little promontories present an obstacle to the progressive movement of the glacier,—than towards the centre and along uniform walls.

M. Agassiz quotes many facts, which seem to him to prove that the crevasses generally do not traverse the glacier, as is supposed; and that the water which accumulates there runs off, by being infiltrated into the ice. In order to place this infiltration beyond a doubt, M. Agassiz lately performed an experiment on a large scale. In a mass of ice included between two great crevasses with very smooth sides, of a deep blue colour and extremely compact, he caused to be excavated a horizontal gallery, 4 feet high by 3 feet wide and 8 feet long. At the surface of the glacier, above the gallery, he bored a vertical hole 5 feet deep, into which he emptied five litres of concentrated tincture of logwood. In half an hour the liquid had all escaped, and, two hours afterwards, it exuded through the capillary fissures along the roof of the gallery, having penetrated a mass of ice of 20 feet. The colour, moreover, extended to the walls of the crevasses, and penetrated below the roof to unknown depths. M. Agassiz has repeated the same experiment a great many times, on a small scale, at different parts of the glacier, and has every where found that the infiltration is much more rapid in the blue than in the white ice; which latter becomes coloured very slowly. An important observation is, that the liquid is not distributed uniformly through the whole mass, but only infiltrates through the capillary fissures.

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* See this Number of Journal, p. 273.

In examining in detail the structure of the ice, M. Agassiz noticed, round the bubbles of air it contains, bubbles of water of various forms, which can only be distinguished in certain positions opposite the light. The presence of this liquid water round bubbles of air in great masses of ice is a very extraordinary fact, and is considered by M. Agassiz as a phenomenon of *diathermansie,* the more especially as these bubbles become larger and more distinct when the ice has been for a long time exposed to the air.

M. Agassiz was anxious to know exactly the quantity of air contained in the various modifications of the ice of the glacier. M. Nicolet took charge of the experiment, and obtained the following mean results:—

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| 500 grammes of snow passing into névé, | 32 cubic centimètres of air. |
| ...................... ice formed under that snow, | 0.9 .............................. |
| ...................... white ice, . . . | 7.5 .............................. |
| ...................... blue ice, . . . | 0.5 .............................. |
| ...................... blue ice of the gallery, | 0.9 .............................. |

M. Agassiz has ascertained, that the nocturnal radiation of the ice is very considerable. It is only in stormy and snowy nights that the thermometrographs placed at the surface of the glacier and of the moraine do not differ in their indications, whereas in clear nights the thermometer always descends 1 or 2 degrees cent. lower on the glacier than on the moraine. M. Hugi asserted, that the temperature of the moraine is always much lower than that of the glacier; observations continued for three weeks have proved the contrary.

It has been long said, that the ice of the interior of the glacier is completely free from earthy matter, because it rejects every thing which falls into its crevasses. This assertion is by no means correct, as the following will show: M. Agassiz melted a quantity of ice raised from a depth of 20 feet under the surface of the glacier, and which yielded 27 litres of water; and he found that it contained 64 grammes of fine sand. Proceeding on these data, we may calculate approximatively the amount of sand contained in the glacier of the Aar (whose ice seems extremely pure), at the enormous quantity of 2,560,000 kilogrammes.

The mode of disaggregation of the ice at the surface of the glacier has also been the object of continued observation. In proportion as the atmosphere acts on the glacier, after the melting of the snows of the cold season, which disappear completely in May and June, the ice becomes porous, but does not decompose uniformly. At first it is generally white, wherever there is no accumulation of fragments of rock and of dust to protect it from the action of the sun; but in proportion as it imbibes the water of the summer rains, its tint becomes more and more blue. These differences of colour are maintained at all parts of the glacier where the form of the surface causes constant currents of water during the day, or at least a larger supply of water after heavy rains. This contrast is particularly striking when a violent fall of rain occurs after a succession of several fine days; the glacier, which was rendered white by the warm days, then becomes at once distinctly blue. When the heat continues for a long time, the whole surface becomes disaggregated in various ways; the white bands assume the aspect of a granular snow, perfectly similar to the névé, whereas the blue bands are decomposed into angular fragments, and the portions which consist of intimately blended blue and white ice assume a structure similar to that of pumice. Another effect of the superficial decomposition of the ice, is the disjunction of the blue and white bands, between which are formed very elongated longitudinal fissures, that penetrate more or less deeply. These fissures frequently give rise to dislocations resembling parallel faults; the whole glacier sometimes acquires, in consequence of these dislocations, the appearance of a great book placed on its back, and so far opened as to cause the leaves to slide on one another.

M. Agassiz next speaks of a curious phenomenon which came under his observation. At half-past four in the evening the workmen were boring, when the glacier began to crack under their feet, and to disengage a large quantity of air-bubbles. Crevices of some lines in breadth soon presented themselves at the surface. After the lapse of some minutes a crack was heard, resembling simultaneous detonations of fire-arms in platoon firing, accompanied by single reports, and commotions similar to those caused by an earthquake. The glacier really trembled. A little afterwards, about seven o'clock, the bore, which was 130 feet in depth and 6 inches in diameter, and full of water, was emptied in a few minutes—a fact proving that these crevices, although very narrow, penetrated to great depths. At half-past eight the shocks still continued, and they were heard during the night. M. Agassiz counted a dozen of crevices, of which the largest was about an inch and a-half wide. A circumstance worthy of notice is, that they all succeeded each other from above downwards, following the slope of the glacier.

With the viewing of obtaining information regarding the temperature of the glacier during winter, M. Agassiz had last autumn introduced two of Bunten's thermometrographs into bores of 12 and 24 feet in depth. (See this Number of the Journal, p. 277.) But notwithstanding the precautions which had been taken, the one already brought up has not afforded a correct result. It was hoped that the other would be extracted by means of a current of water.

In conclusion, M. Agassiz announces the return of MM. Desor and Escher de la Linth from a successful ascent of the Schreckhorn, whose summit rises to the height of 4082 mètres, and had not previously been reached. The hygrometer indicated 43 at + 4°. C. (39°. 2 F.)—(*L'Institut, No.* 453).

## PROCEEDINGS OF THE ROYAL SCOTTISH SOCIETY OF ARTS, SESSION 1841–42.

THE Annual General Meeting of the SOCIETY was held in the Royal Institution, on Monday, 8th November 1841,—Andrew Fyfe, M.D., F.R.S.E., President, in the Chair.

Before proceeding to the business of the evening, Dr Fyfe addressed the Members. He congratulated them on having, through the exertions of their Secretary, obtained a Royal Charter of Incorporation, and that in future the Society is to bear the title of "THE ROYAL SCOTTISH SOCIETY OF ARTS." During the three Sessions preceding that at which he was called to the Chair, the number of entrants amounted on an average to 45 per annum. In the last Session the number was 39; while, at the same time, numerous communications were presented, some of them of great importance, not only in a scientific, but also in a practical point of view, as was shewn by the printed Report of the Prize Committee, in which it would be seen that a greater number of Premiums are this year awarded than during any former Session.

Dr Fyfe afterwards alluded to the loss the Society had sustained by the death of several of its distinguished members, but which, he hoped, would be compensated for by the admission of others, and the renewed exertions of those he now addressed; and he entreated them that, whatever differences of opinion might exist among them, they would carry on their discussions in the spirit of charity towards one another, bearing in mind that the discovery of *truth*, and not *victory*, was the object they had in view; endeavouring to assist each other in their pursuits, and thus, by co-operation, striving to promote the welfare of the Society, and consequently giving encouragement to the Arts and Sciences, the object for which the Society was founded.

The following communications were then made:—

1. At the request of the COUNCIL, an Experimental Exposition of the Theory and Mechanism of the Steam-Engine was given by George Glover, Esq., F.R.S.S.A., Lecturer on Natural Philosophy and Medical Physics, Edinburgh. (829.)

Mr Glover illustrated the subject by a variety of tables, diagrams, and experiments, exhibiting the various contributions which have been made to our knowledge of the expansive effects of heat on elastic fluids, more especially of steam; referring to the investigations of the late Professors Robison and Black; of Dalton and Gay Lussac on the laws of expansion, and the recent corrections by Rudsberg; the experiments of Ure and Southern, and the Report by the Committee of the French Institute, on the elastic force of high pressure steam. Mr Glover then traced the history of the steam-engine from the early records of its employment in the idolatrous worship of the Egyptians and Greeks, down to its perfection by the genius of Watt,—alluding curiously to the various modifications it has since undergone.

Thanks voted, and given to Mr Glover from the Chair.

II. The Report of the Prize Committee, awarding the Prizes for Session 1840–41, was read; and the Prizes were delivered by the President to the successful Candidates.

III. The Models, Drawings, &c. of Inventions, &c., for which the Prizes have been awarded were exhibited.

### PRIVATE BUSINESS.

I. The following Candidates were balloted for as Ordinary Fellows, viz.:—

1. Douglas Baird, Esq., Ironmaster, Gartsherrie. 2. David Thomson Hope, Esq., Civil Engineer, 8 Russel Street, Liverpool.

II. Lists of the Fellows as at 1st November 1841, were laid on the Table; and are to be circulated with the Fasciculus of the Transactions about to be issued.

III. The Society elected the following Office-Bearers for Session 1841–42:—

The QUEEN, *Patroness.*

Sir JOHN ROBISON, K.H., F. R. S. E., *President.*
JOHN SHANK MORE, Esq., F.R.S.E. } *Vice-Presidents.*
WILLIAM GALBRAITH, A. M., }
JAMES TOD, Esq., W. S., *Secretary.*
JOHN SCOTT MONCRIEFF, Esq., Accountant, *Treasurer.*

*Ordinary Councillors.*

C. H. WILSON, A. R. S. A.
ALEX. BRYSON.
ALEX. ROSE.
W. CRAWFURD.
CHARLES COWAN.
WILLIAM WOOD.
ANDREW TAWSE.
DOUGLAS MACLAGAN, M. D.
ANDREW FYFE, M. D.
GEORGE GLOVER.
ROBERT WRIGHT.
ARCHD. GIBSON.

*Editor of Transactions*,—M. PONTON, Esq., F. R. S. E.,
*Curator of Museum*, (Vacant till new arrangements be made.)

*22d November* 1841.—Sir John Robison, K.H., President, in the Chair.

Before commencing the business of the evening, the President expressed the gratification he felt in being placed in the Chair of a Society in the welfare of which he had always taken a warm interest, having co-operated with Sir D. Brewster and Mr Guthrie Wright in its early organization, and having accompanied it in its progress from small beginnings to the state of vigour and efficiency which it had now attained; a state of prosperity, he observed, which was in a great measure owing to the unwearied assiduity of their Secretary, and to the zeal and talent of the successive Councils, and of his predecessors in the Chair, to whom the Society had intrusted the management of their affairs. He added a hope that the progressive advance which had been made by the Society would be continued, and that, with the augmented numbers and renewed activity of the members, they might anticipate a copious supply of valuable communications, from which large additions might be gained to their stores of useful knowledge. The President then proceeded to add, that many well-informed persons doubted the utility of such Institutions in promoting the useful arts. These persons enquire, "Where are the great inventions or improvements which such societies have produced?" To such persons he suggested it might be replied, that their view of the effects of such societies was a very limited one, and that they might with as little justice assert, that a whet-stone was of no use, because it is not applied directly to the fabrication of machinery; yet without this whet-stone to prepare his instruments, the mechanic could not construct any of those machines which had raised Britain to its eminent rank among nations. It might not often be the interest of individuals to make such societies the channels by which their discoveries or improvements should be brought before the public; but it did not follow that the first germs of their discoveries were not sown in such meetings, although the blossom and the fruit might appear elsewhere; their meetings might be looked on as whet-stones of the intellect, and few persons who attended them would say, that, in listening to the communications read at them they had not gained useful knowledge, or would deny that, in taking part in the discussions arising from this, valuable ideas had been excited in their own minds, which, but for such excitement, might have lain for ever dormant. He concluded by stating, that, in an account given in the London papers of the 19th, of the rendezvous of the fleet of West India steam packets at Southampton, there was a striking confirmation of the view he had taken of the effect of such societies. In this account it is stated, "that these vessels, as they arrive at the rendezvous, are subjected to certain trials of their qualities, and that three (which are there named) have proved very superior to the others." The President observed, that it so happened, that these three were constructed by the same person, a member of their own Society, who had discovered a new principle in naval architecture, which principle had been tested by an extensive series of experiments, made at the expense of the British Association for the Advancement of Science. These circumstances, he considered, afforded undeniable evidence of the positive good which is produced by the exertions of such societies, and an ample refutation of the doubts expressed of their usefulness.

The following addresses to Her Majesty the Queen, and to his Royal Highness Prince Albert, on occasion of the Birth of the Heir-Apparent to the Crown, proposed by the Council, were unanimously approved of, and ordered to be signed, sealed, and transmitted to the Secretary of State for the Home Department for presentation:—

"Unto the QUEEN'S MOST EXCELLENT MAJESTY,

The Humble ADDRESS of the ROYAL SCOTTISH SOCIETY OF ARTS.

"May it please your Majesty,

"We, the President and Fellows of *The Royal Scottish Society of Arts*, recently incorporated by your Majesty's Royal Charter, beg leave, as our first Public Act under our Charter, respectfully to approach your Majesty with the assurance of our sincere attachment to your Royal Person and Government, and to express loyally and fervently our participation in the great joy with which the auspicious birth of a Prince has filled all hearts. United, as we are, for the encouragement and promotion of the *Useful Arts*, we gratefully acknowledge the advantages which the benignant influence of our happy Constitution, under your Majesty's gracious sway, has ever afforded for the prosecution of useful study, and the advancement of art and science. With feelings, therefore, of deep emotion, we regard the present most propitious event, which gives to this Empire another security, that, under the royal shelter and patronage of your House, the cause of Science and the Arts shall long continue to flourish in tranquil prosperity. Most heartily we pray that a gracious Providence may surround your Royal Person and Family with every blessing, and may long preserve your Majesty in the fulness of domestic happiness and of public glory to reign over a free, loyal, and religious people.

"Signed in our name and by our appointment, by the President, Vice-Presidents, Secretary, and Treasurer, and sealed at Edinburgh the twenty-second day of November, in the year one thousand eight hundred and forty-one.

(Signed) "JOHN ROBISON, *President.*
"J. S. MORE, *Vice-President.*
"WILLIAM GALBRAITH, *Vice-President.*
L S "JAMES TOD, *Secretary.*
"JOHN SCOTT MONCRIEFF, *Treasurer.*"

Unto Field-Marshall His Royal Highness PRINCE ALBERT, K.G. &c.

The Humble ADDRESS of the ROYAL SCOTTISH SOCIETY OF ARTS.

"May it please your Royal Highness,

"We, the President and Fellows of *The Royal Scottish Society of Arts*, desire respectfully to express our faithful attachment to your Royal Person, and to offer our humble congratulations to your Royal Highness on the auspicious event, which, by the birth of a Prince, has gladdened the hearts of the people of these realms; and which, in the merciful dispensation of Providence, has preserved unimpaired the health of your Royal Highness's Consort, our most gracious Sovereign. We earnestly pray that the life of the illustrious Prince may be long and prosperous, and that every member of the Royal House, of which we trust he is destined to be a signal ornament, may be ever secure in the loyal affections of a happy people.

"Signed in our name, and by our appointment, by the President, Vice-Presidents, Secretary, and Treasurer, and sealed at Edinburgh the twenty-second day of November, in the year eighteen hundred and forty-one.

L S (Signed as above.)

## The following Communications were then made :—

1. Description, with Diagrams, of a Portable Diorama, constructed by George Tait, Esq. advocate. The Diorama was exhibited. Thanks voted, and abstract to be printed in the Transactions. (833.)

2. Description, with a Drawing, of the Apparatus invented by him for turning on and shutting off the Gas which illuminates the Translucent Clock-Dial above his shop. By Mr Robert Bryson, clock and watchmaker, Prince's Street, Edinburgh. A full-sized working Model was exhibited. Thanks voted, and to be printed in the Transactions. (834.)

3. Donation.—Astronomical Observations made at the Royal Observatory, Edinburgh. By Thomas Henderson, Esq. F.R.SS.L. and E. &c., Her Majesty's Astronomer for Scotland. Vol. IV. in 1838. Presented by the Author. Thanks voted. (836.)

4. Donation.—The Civil-Engineer and Architect's Journal, Nos. 45, 47, 48. Presented by Mr Knowles. Thanks voted. (825, 826, 827.)

5. Donation.—The Cape of Good Hope Pamphlet, No. 1. (1841.) Presented by Mr H. Dempster, the Author. Thanks voted. (821.)

I. The following Candidates were elected Ordinary Fellows, viz. :—

1. James Saunders Robertson, Esq. W.S., 16 South Charlotte Street.
2. Mr John Hughes, printer, 37 East Claremont Street.
3. Mr Charles Macpherson, printer, 37 George Street.

II. Motion by the Secretary,—" That the sum of L. 15 be given out of the Experimental Fund, in aid of any subscriptions which may be raised towards enabling Mr Davidson of Aberdeen to construct an Engine of any kind on the large scale to be propelled by Electro-Magnetism, for the purpose of discovering the relation which may exist betwixt Size and Power. The money to be expended at the sight of a Committee; but Mr Davidson to be left uncontrolled as to the method to be followed in his experiments."

The motion was carried unanimously.

III. In terms of Law XX., the Society appointed a Committee to audit the Treasurer's Books, and to report thereon, and generally on the state of the Funds of the Society. The Report to be given in at the meeting on 13th December next. The books will be laid on the table.

IV. It was stated that a Fasciculus, containing Part V. of Vol. I., and Part I. of Vol. II. of the Society's Transactions, would speedily be delivered by the Collector to Ordinary Fellows residing in Edinburgh. Those residing in the country were desired to apply for their copies to the Treasurer.

13*th December* 1841.—Sir John Robison, K.H. President, in the Chair. The following Communications were made :—

1. On a new form of Roofing Tile, recently invented in France, uniting the advantages of more perfect protection from the weather, with a better appearance and less weight than those now in use. Communicated by Sir John Robison, F.R.S.E., Pres. R.S.S. Arts.

Specimens of the Tiles and a Model of a Roof were exhibited. Thanks voted, and description to be printed in the Transactions. (839.)

2. Description and Drawing of a Compensation-Governor for the Steam Engine. By Mr John Yule, 47 Sauchiehall Street, Glasgow. Communicated by John Scott Russell, Esq. F.R.S.S.A. Greenock. Referred to a committee. (819.)

3. Description and Drawings of Improved Expanding Screw-Tap, Drill, and Mandrill. By the same. Communicated by Mr Scott Russell.

The Screw-Tap, Drill, and Mandrill, were exhibited. Referred to a committee. (820.)

4. The Secretary read a paper, on the Economy of raising Water from Coal Mines on the Cornish principle. Communicated by William Fairbairn Esq. engineer, Manchester, Hon. M.R.S.S.A. (838.)

The following donation was laid on the table. :—

Transactions of the Manchester Geological Society, Vol. I. 1841. Presented by William Fairbairn, Esq. engineer, Manchester, Hon. M.R.S.S.A. (838.)

The following Candidates were elected as Ordinary Fellows, viz. :—

1. Mr John Stevenson Brown, iron merchant, 17 George Square.
2. Mr James S. Duncan, bootmaker, 146 Prince's Street.
3. John Steel, Esq. sculptor to her Majesty, 1 Randolph Place.
4. George Tait, Esq. advocate, 4 Abercromby Place.

In terms of a recommendation by the Council, Mr Alexander Kirkwood, Meuse Lane, St Andrew Street (a Fellow of the Society), was appointed Medallist to the Society during pleasure.

The Secretary moved—" That six tickets of admission to each of the ordinary meetings of the Society be placed at the disposal of the Secretary of the Edinburgh School of Arts, to be given to such of the students of that School as the respective lecturers may consider best entitled to them from good attendance and proficiency in their studies."

This motion was carried unanimously, on the understanding that the tickets shall not be issued until larger accommodation be provided for the meetings of the Society.

In terms of Law XX., the report of the Committee appointed to audit the Treasurer's books, and to report thereon, and generally on the state of the funds of the Society, was read and approved of. Mr Horne, Convener.

The President granted discharge to the Treasurer in terms of Law XX.

*10th January* 1842.—Sir John Robison, K.H., President, in the Chair.

An Answer was read from the Secretary of State for the Home Department, to the Society's Address to her Majesty the Queen on the birth of the Prince of Wales, stating that her Majesty had been pleased to receive the same very graciously.

An Answer was also read from G. E. Anson, Esq. Treasurer to his Royal Highness Prince Albert, to the Society's Address to his Royal Highness.

The following Communications were then made :—

1. A Model of an Apparatus for Tilting or Emptying Waggons at the termination of Railways was exhibited and described by James Thomson, Esq., Civil Engineer, Glasgow, F.R.S.S.A.—Thanks voted, and referred to a committee; and Mr Thomson was requested to furnish a written description, with a diagram, for the Society's Transactions. (840.)

2. On the use of Chlorine for ascertaining the illuminating power of Coal-Gas; and on the comparative expense of Light derived from different sources. By Andrew Fyfe, M.D., F.R.S.E., and F.R.S.S.A. The Bude Light and some others were shewn.—Thanks voted, and abstract to be printed in the Transactions. (846.)

The author first alluded to experiments made some years ago, in which he proposed Chlorine as a means of ascertaining the illuminating power of Coal-Gas, and he again adverted to the subject, owing to his having been lately engaged in an enquiry as to the comparative illuminating power of gases derived from different sources. In these trials, in addition to the usual test, by the shadow, he had again recourse to the action of chlorine, and in all his experiments he found the results to agree so nearly, that he conceives that chlorine may be employed with the most implicit confidence for indicating the illuminating power; which may he stated to be just as the amount of condensation. He afterwards related the results of trials made with the view of finding the comparative expense of light derived from other sources, —as from a variety of candles made of tallow, wax, and other substances, and also from oils, consumed both in common lamps and in others with contrivances adapted to them for the more perfect combustion of the oil. In all of these, he found the light from gas by far the cheapest. Next came the Solar lamp, and Naphtha, then the oils in Argand lamps, and lastly the candles, of which the tallow were the least expensive, the wax and spermaceti the most so. He also stated that the comparative expense of these would depend on the quality of the gas, which varies much in different parts of the Kingdom, and hence the value of the chlorine test, by which the illuminating power of different gases could be easily ascertained.

3. On a method of softening the tone of the Clarionet for chamber practice. By Mr William Meikle, Town-end, Strathaven. Muted clarionets,

on different keys, were exhibited.—Thanks voted, and referred to a Committee. (844.)

4. Description of a Sailing Vessel on a new construction and Rig, capable of performing a greater number of evolutions, and more rapidly, than vessels on the common construction; the Rig being safe, powerful, and handy. By Mr Henry Dempster, Mariner, Kinghorn. A model of the vessel was exhibited.—Thanks voted, and referred to a Committee. (845.)

The following Donations have been received:—

1. Specimen of Jacquard Loom-Weaving; being a copy of an engraving from Steuben's picture of the Emperor Peter the Great in a fisher's boat in a storm; woven in silk with a warp of inferior quality, at the imperial manufactory of Alexandrosch, near St Petersburg. Presented by General Alexander Wilson, Hon. M.R.S.S.A.—Thanks voted. (830.)

2. Printed Description of the Monocleid Writing Cabinet, invented by Mr Thos. Sopwith, C.E. Presented by Mr John Sopwith, Newcastle-on-Tyne.—Thanks voted. (823.)

3. Printed Description and Diagrams of Tayler's patent Floating Breakwater, and Prospectus of the National Floating Breakwater and Refuge Harbour Company. (1841.) Presented by James Saunders Robertson, Esq. W.S., F.R.S.S.A.—Thanks voted. (847.)

### PRIVATE BUSINESS.

I. The following Candidates were balloted for and elected as Ordinary Fellows, viz.:—

1. Mr John Taylor, Cabinet and Picture-frame maker, 55 George Street.
2. William Drysdale, Esq. Assistant Clerk of Session, 3 Hart Street.

II. Mr Samuel Leith, Lithographer, South St Andrew Street, Associate, having expressed his desire to become an Ordinary Fellow in terms of Law V. was enrolled accordingly

*24th January* 1842.—William Galbraith, A.M., F.R.A.S., Vice-President, in the Chair. The following Communications were made:—

1. Part First. On the variable Specific Gravity of the Human Body, and on some new principles considered in the mechanism of Drowning. By George Glover, Esq., F.R.S.S.A., Lecturer on Natural Philosophy, Edinburgh.—Thanks voted. (856.)

Part Second to be given on a future occasion.

2. Specimen of Scotch Ultra-Marine. By Mr Murdoch Paterson, dyer Inverness. Referred to a Committee. (828.)

3. An account of M. Triger's method of Sinking Pits in moveable or watery sands on the Loire, by means of compressed air. Translated from Report of the Meeting of the French Academy of Sciences of 2d November 1841, by J. Mitchell, Esq., Leith. Communicated by Wm. Galbraith, A.M., V.P.R.S.S.A.—Thanks voted. (841.)

4. Description, with a Drawing, of a Self-acting Stopper for Winding-Engines. By Mr John Maxton, engineer, Greenock. Communicated by John Scott Russell, Esq., F.R.SS.A., Greenock.—Referred to a Committee. (835.)

5. Dr Fyfe made a few additional observations on the subject of his Paper on the comparative expense of Light from Gas, Oil, Candles, &c. (846.)

6. Proposal for a System of Signals to prevent collision upon Railways. By James Robertson, Esq., civil and mining engineer, Edinburgh.—Referred to a Committee. (849.)

7. On his application, permission was given to Mr H. Dempster to withdraw for the present his Sailing Vessel on a new construction and rig. (845.)

The following Donations were laid on the table :—

1. Lectures on Agricultural Chemistry and Geology, No. V. (July 1841.) By Jas. F. W. Johnston, M.A. University, Durham. Presented by the Author.—Thanks voted. (824.)

2. The Civil Engineer and Architect's Journal, Nos. 50 and 51. Presented by Mr Knowles.—Thanks voted. (843.)

PRIVATE BUSINESS.

I. The following Candidates were elected Ordinary Fellows, viz.:—

1. John Gillespie, Esq., 44 Castle Street.
2. Mr Alexander Kay, plumber, 144 Prince's Street.

14*th February* 1842.—Sir John Robison, K.H., President, in the Chair. The following Communications were made :—

1. Part Second of Mr George Glover's communication on Drowning. (856.) Thanks voted.—Part Third, on the best means of Resuscitation, to be given at next meeting.

2. Description of a simple and accurate Instrument, adapted to the formation of fine hinges. By Mr John Shearer, 84 George Street, Edinburgh. The tool was exhibited, and its application shewn. Thanks voted. (855.)

3. A Safety-Cape, &c., for skaters and others frequenting the ice, or travelling by water, were exhibited and described by James Simpson, Esq. advocate. Thanks voted. (865.)

4. Specimens of Sheet Caoutchouc, prepared in London by an improved process, were exhibited by Mr James Dowie, boot-maker, London and Edinburgh, F.R.S.S.A. Thanks voted. (854.)

5. The Oxyhydrogen Blow-pipe and other forms of Blow-pipes suited to the purposes of jewellers, goldsmiths, cutlers, and other artists, and for being used with coal-gas along with atmospheric air, were exhibited by Sir John Robison, K.H., President R.S S.A., Mr George Glover, and Mr William Kirkwood, plumber, Thistle Lane, Edinburgh. The Blow-pipes of Sir John Robison and Mr Kirkwood, fitted for being used with coal-gas and common air, were considered to be well adapted, not only for soldering, but for the

tempering of steel tools, as it can be seen when the steel acquires the proper temperature, and thus rendering it unnecessary to bring back the temper, which is very hurtful in many cases. Thanks voted to the exhibitors. (866, 867, 868.)

6. Major Playfair exhibited some beautiful specimens of Daguerreotype Portraits, executed by M. Claudet, at the Adelaide Gallery, London, by the Refracting Camera. Thanks voted. (869.)

The following Donations were laid on the table :—

1. The Civil Engineer and Architect's Journal, Nos. 46, 49, and 52. Presented by Mr Knowles. (850.)
2. Report of a Committee to the Lord Mayor, &c. of London, on the nuisance arising from the Smoke of Manufactories and Steam-Engines. Presented by Henry Woodthorpe, Esq. (852.)
3. Trigonometrical Surveying, Levelling, and Railway Engineering. By William Galbraith, M.A., F.R.S.S.A., F.R.A.S. 1842. Presented by the Author. (861.)

Thanks voted to the donors.

### PRIVATE BUSINESS.

The following Candidates were balloted for, and elected as Fellows, viz. :—

1. Mr Paul Crawford, agent for Falkirk Iron Company, 21 Geo. IV. Bridge.
2. Mr Robert Muller, artist and pianist, 27 George Street.
3. Mr William Cushnie, Malta Green.
4. Mr William Croall, coach-builder, 21 Broughton Street.

II. On the motion of the Council, the following distinguished foreigners were elected as Honorary Members, viz.:—

1. M. le Baron Thenard, Membre de la Sociéte d'Encouragement pour l'Industrie Nationale de Paris.
2. M. le Baron Seguier, do. do.
3. M. Pouillet, Membre de l'Academie des Sciéncés de l'Institut, Prof. de Physique au Conservatoire des Arts et Metiers.

*28th February* 1842.—Sir John Robison, K.H., President, in the Chair. The following Communications were made :—

1. Description, with Drawings, of a Brine-Gauge, or Salinometer, for indicating the degree of saturation of the brine of Marine-Boilers. Invented by John Scott Russell, A.M., F.R.S.S.A., civil engineer, Greenock. Thanks voted, and to be printed in the Transactions. (876.)
2. On Improvements of the Solar and Oxyhydrogen Microscope, Polariscope, &c. By Mr Thomas Davidson, optician, Edinburgh, F.R.S.S.A. Referred to a committee. (857.)
3. Third Notice on the Mechanical Arts of Persia, including Carpentry,

Smith-work, Turning, Stone-cutting, Mills, and Water-works. By James Robertson, Esq., civil and mining engineer, Edinburgh, late in the service of the Shah of Persia. Drawings were exhibited. Thanks voted. Abstract to be printed in the Transactions. (842.)

4. Some beautiful Specimens of Electrotype Medals, &c., by Mr Mure of Glasgow, were exhibited by Mr Bryson. Thanks voted. (880.)

The following Donations were laid on the table :—

1. New Tables of the Elements of Catadioptric Zones for Lights of the first order. Calculated by Alan Stevenson, LL.B., F.R.S.E., civil engineer, Edinburgh. Presented by the Author. (863.)

2. Description of a Cofferdam adapted to a hard bottom ; used in excavating rock from the navigable channel of the River Ribble. By David Stevenson, Esq., F.R.S.S.A., civil engineer, Edinburgh. Presented by the Author. (864.)

3. Registration of Designs in order to secure Copyrights. By Mr William Carpmael, Lincoln's Inn. (London, 1842.) Presented by Thomas Weir, Esq., W.S. (869.)

Thanks were voted to the donors.

### PRIVATE BUSINESS.

The following Candidates were balloted for, and elected as Fellows, viz. :—

1. Mr John Dickman, watchmaker, 142 George Street.

2. Mr Joseph Martin Kronheim, ornamental designer, 9 Buccleuch Place.

## *List of Patents granted for Scotland from 29th June to 20th September* 1842.

1. To John Americus Fanshawe of Hatfield Street, in the parish of Christ-Church, in the county of Surrey, gentleman, " an improved manufacture of waterproof material applicable to the purposes of covering and protecting surfaces, bodies, buildings, and goods, exposed to water and damp."—29th June 1842.

2. To James Boydell junior, of the Oak Farm Works, near Dudley, in the county of Stafford, iron-master, " improvements in the manufacture of keel-plates for vessels, iron-gates, gate-posts, fencings, and gratings."—30th June 1842.

3. To Michael Coupland of Pond Yard, Park Street, Southwark, millwright and engineer, " improvements in furnaces."—30th June 1842.

4. To Thomas Banks of Manchester, in the county of Lancaster, engineer, " certain improvements in the construction of wheels and tyres of wheels to be employed upon railways."—5th July 1842.

5. To John Tresahar Jeffree of Blackwall, in the county of Middlesex, engineer, " certain improvements in lifting and forcing water and other fluids, parts of which improvements are applicable to steam-engines."—6th July 1842.

6. To James Nasmyth of Patricroft, near Manchester, in the county of

Lancaster, engineer, "certain improvements in machinery, or apparatus for forging, stamping, and cutting iron and other substances."—7th July 1842.

7. To Charles Augustus Preller of East Cheap, in the city of London, merchant, being a communication from abroad, "improvements in machinery for preparing, combing, and drawing wool and goats' hair."—13th July 1842.

8. To William Revell Vigers of Russell Square, in the county of Middlesex, Esquire, being a communication from abroad, "a mode of keeping the air in confined places in a pure or respirable state to enable persons to remain or work under water, and in other places, without a constant supply of fresh atmospheric air."—13th July 1842.

9. To Gottlieb Boccius of the New Road, Shepherd's Bush, in the county of Middlesex, gentleman, "certain improvements in gas, and on the methods in use, or burners, for the combustion of gas."—14th July 1842.

10. To John Hall of Breezes Hill, Ratcliff Highway, in the county of Middlesex, sugar-refiner, "improvements in the construction of boilers for generating steam."—18th July 1842.

11. To John Elliott Fox of Finsbury Circus, in the city of London, gentleman, being a communication from abroad, "improvements in steam-engines."—18th July 1842.

12. To William Newton of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, being a communication from abroad, "certain improved machinery for excavating, dredging, and removing earthy and stony matters in the construction of railroads, canals, cleaning of rivers, harbours, and redeeming of marshy or alluvial soils; also for boring rocks, indurated clay, and other earthy matters, for the purpose of blasting and removing the same, the whole to be worked by steam and other power."—25th July 1842.

13. To Thomas Hendry of Glasgow, Scotland, mechanic, "certain improvements in machinery for preparing and combing wool, and other fibrous materials."—27th July 1842.

14. To Thomas Waterhouse of Edgeley, in the county of Chester, manufacturer, "a certain improvement or improvements in machinery used for carding, drawing, and roving cotton, wool, flax, silk, and other similar fibrous material."—27th July 1842.

15. To John Osbaldeston of Blackburn, in the county of Lancaster, metal heald-maker, "improvements in looms for weaving."—29th July 1842.

16. To William Geeves of Old Cavendish Street, in the county of Middlesex, gentleman, "improvements in machinery for cutting cork."—29th July 1842.

17. To John Woodcock of Manchester, in the county of Lancaster, mill-wright, "certain improvements in the construction of steam-engines."—1st August 1842.

18. To Alexander Johnston of Hillhouse, in the county of Edinburgh, Esquire, "improvements on carriages, which may also be applied to ships, boats, and various other purposes where locomotion is required."—2d August 1842.

19. To Julius Seybell of Golden Square, Westminster, in the county of Middlesex, manufacturing chemist, "certain improvements in the manufacture of sulphate of soda and chlorine."—11th August 1842.

20. To **Benjamin Biram** of Wentworth, in the county of York, colliery-viewer, "certain improvements in the construction and application of rotary engines."—11th August 1842.

21. To **William Hancock** the younger, of Amwell Street, in the county of Middlesex, gentleman, "certain improvements in combs and brushes."—16th August 1842.

22. To **John Anthony Tielens** of Fenchurch Street, in the city of London, merchant, being a communication from abroad, "improvements in machinery or apparatus for knitting."—22d August 1842.

23. To **Job Cutler** of Lady Pool Lane, in the borough of Birmingham, gentleman, "improvements in the construction of tubular flues for steam-boilers, and in the manufacture of tubes for such and other purposes."—23d August 1842.

24. To **Henry Barclay** of Bedford Row, in the county of Middlesex, dentist, "a composition or compositions applicable as tools or instruments for cutting, grinding or polishing glass, porcelain, stones, metals, and other hard substances."—25th August 1842.

25. To **William Edward Newton** of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, being a communication from abroad, "improvements in machinery or apparatus for making or manufacturing screws, screw-blanks and rivets."—31st August 1842.

26. To **Eugene de Varroc** of Bryanstone Street, Portman Square, in the county of Middlesex, gentleman, "apparatus to be applied to chimneys to prevent their taking fire, and for rendering sweeping of chimneys unnecessary."—1st September 1842.

27. To **Thomas Marsden** of Salford, in the county of Lancaster, machine-maker, and **Solomon Robinson** of the same place, flax-dresser, "improvements in machinery for dressing or hackling flax and hemp."—1st September 1842.

28. To **Samuel Morand** of Manchester, merchant, "improvements in machinery or apparatus for stretching fabrics."—1st September 1842.

29. To **William Henry Kempton** of South Street, Pentonville, in the county of Middlesex, gentleman, "improvements in the manufacture of candles."—2d September 1842.

30. To **John George Hughes** of No. 158 Strand, in the county of Middlesex, general agent, "a new application of telegraphic signals, and the mode of applying the same."—2d September 1842.

31. To **Joseph Whitworth** of Manchester, in the county of Lancaster, engineer, "certain improvements in machinery or apparatus for cleaning roads, and which machinery is also applicable to other similar purposes."——2d September 1842.

32. To **John Thomas Betts** of Smithfield Bars, in the city of London, gentleman, being a communication from abroad, "improvements in covering and stopping the necks of bottles and other vessels."—8th September 1842.

33. To **Isham Baggs** of Wharton Street, in the county of Middlesex, chemist, "improvements in obtaining motive power by means of carbonic acid."—8th September 1842.

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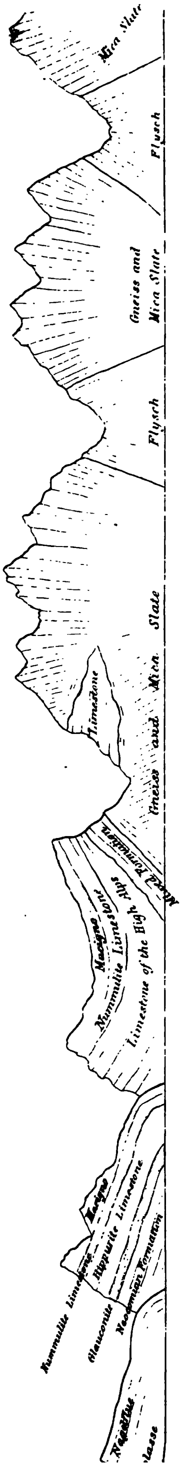
IDEAL SECTION OF THE ALPINE SYSTEM.



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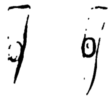

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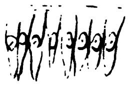

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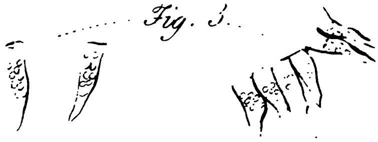

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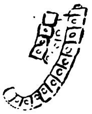

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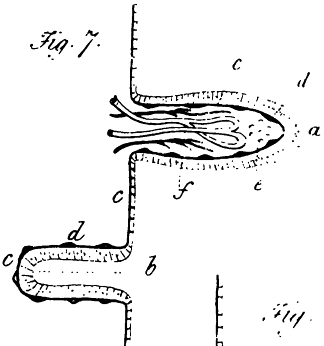

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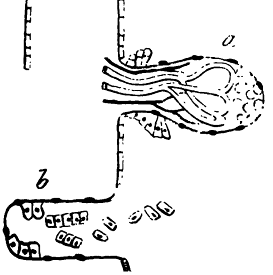

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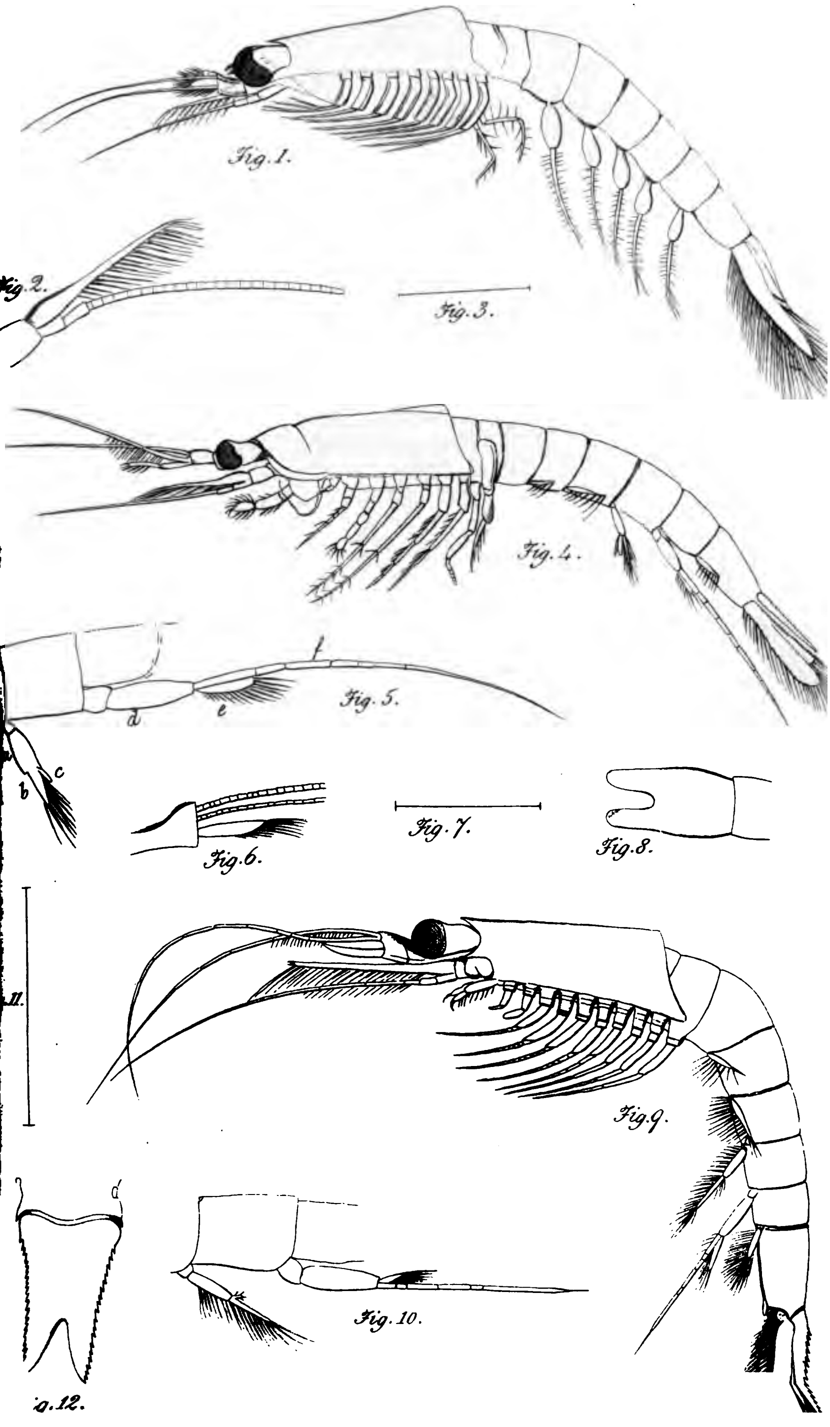

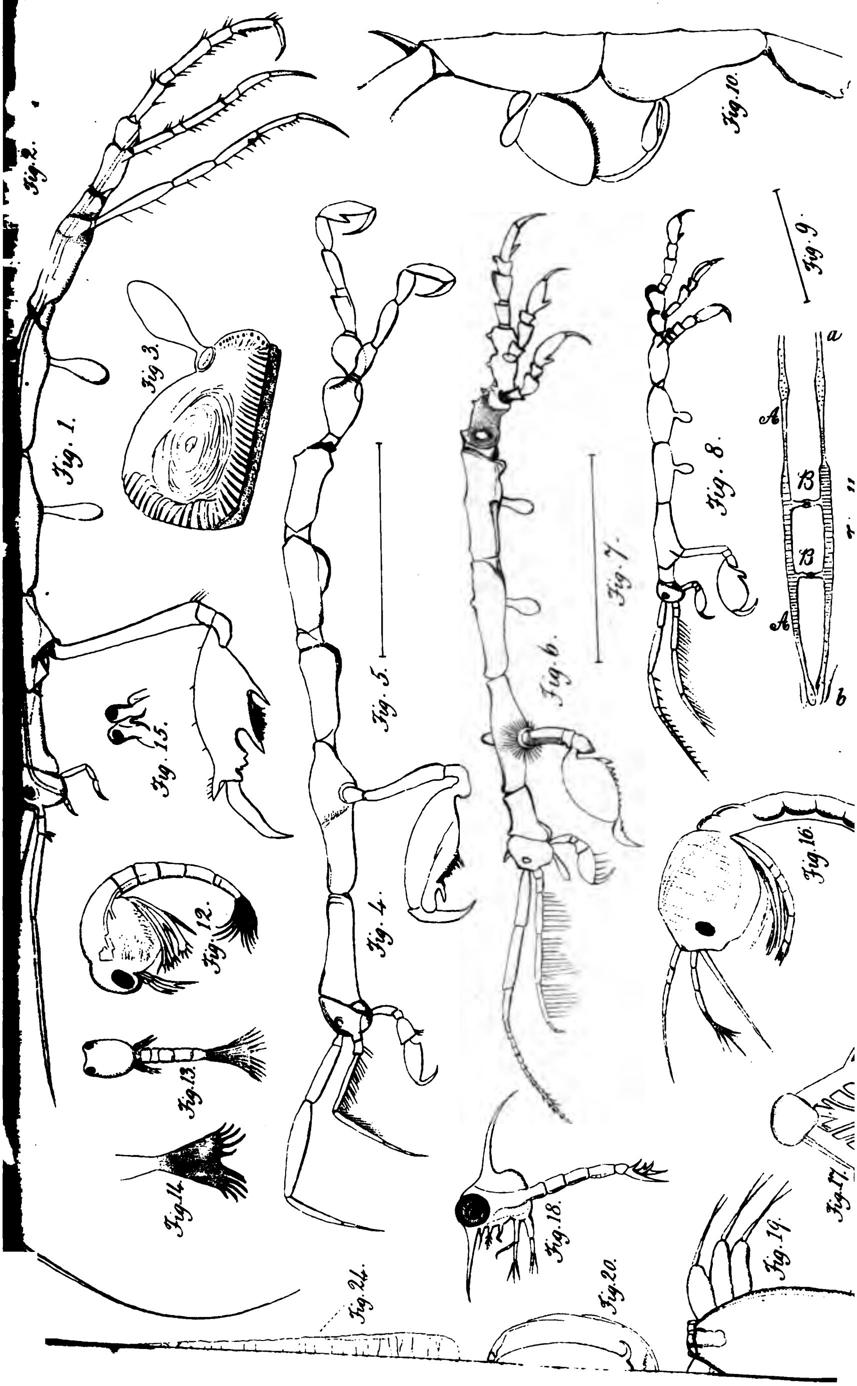
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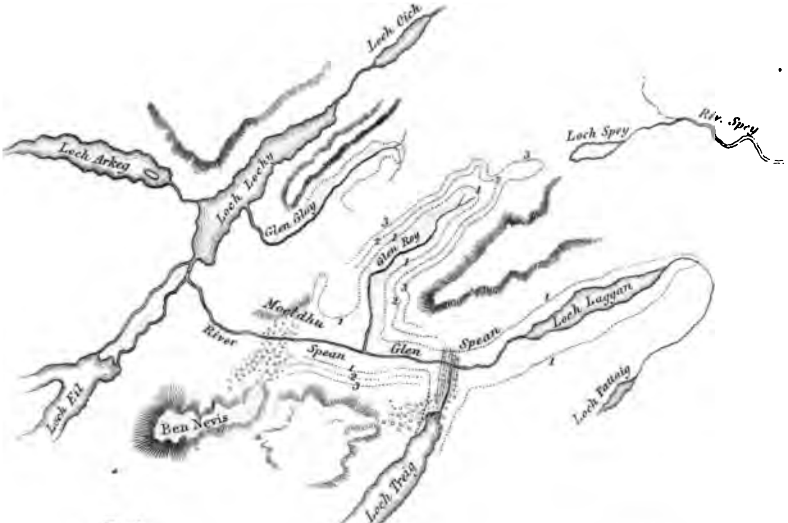


Fig. 1.

........ 1. 2. & 3. *The Parallel Roads.*

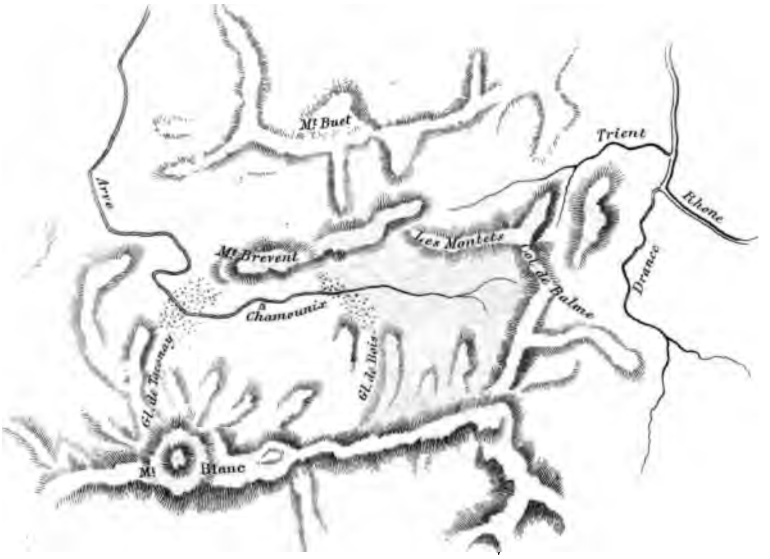
⁘ *Supposed Glaciers.*

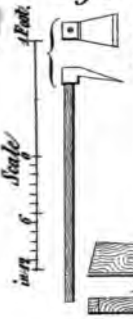
||| *Striae of the Polished Rock.*

Fig. 2.

⁘ *Supposed prolongations of the Glaciers de Taconay & de Bois.*

Fig. 2.





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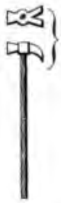

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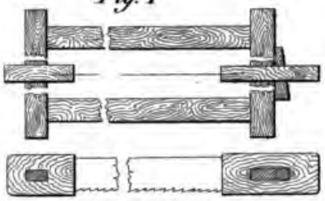

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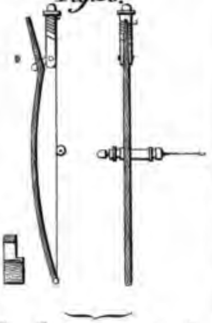

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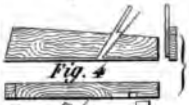

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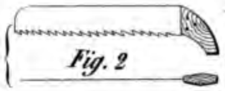

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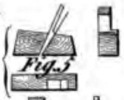

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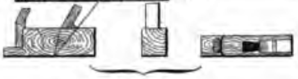

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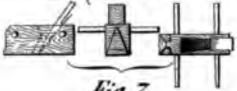

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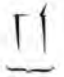

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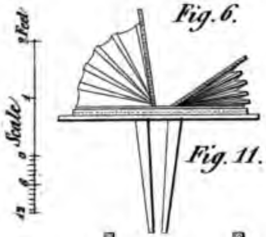

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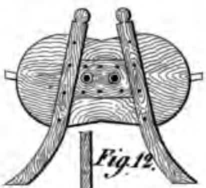

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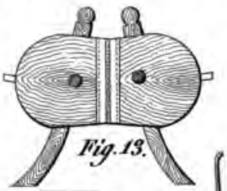

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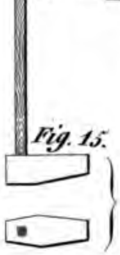

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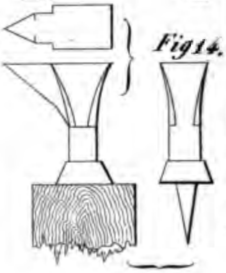

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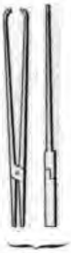

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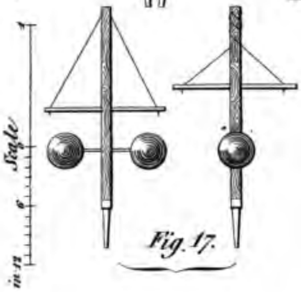

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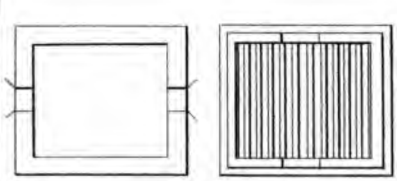

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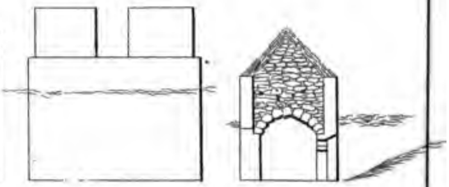

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